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THE

JOURNAL OF SCIENCE.

DECEMBER, 1881.

I. SOME RESULTS OF GRAVITATION.

By CHARLES MORRIS.

IN considering the general results of the Newtonian law of gravitation we arrive at an interesting deduction, which it may be well to briefly describe. This is that, if we view the matter of the universe surrounding any central particle as made up of successive spherical shells, constantly increasing in diameter outwardly, then this particle must exert a precisely equal attraction upon each of these shells, no matter what its diameter, provided they be of equal density and of inappreciable thickness.

Such a result necessarily flows from the law of diminution of attractive energy with distance. The force of gravity diminishes in the ratio of the inverse square of distance. At twice the distance the gravity is reduced to one-fourth. At three times the distance to one-ninth. But at twice the distance the surface area of such a hollow shell is increased fourfold; at three times the distance ninefold. Thus, as the radii of these shells increase, their surface areas increase in the same proportion as the vigour of attraction diminishes, namely, in the ratio of the square of the radius. If we take two such shells, one with twice the radius of the other, then the attraction of the central particle upon every portion of the surface of the smaller shell will be four times as great as that upon an equal portion of the surface of the larger shell. But the whole surface area of the larger shell is four times that of the smaller. Therefore the vigour of attraction upon this whole surface must just equal that upon the whole surface of the smaller. If the radius of the larger were

three times that of the smaller, then the attraction upon a unit portion of its surface would be reduced to one-ninth. But the area of its surface would be nine times that of the smaller. Consequently the attraction upon the whole surface of each must be the same.

In fact the areas of spherical surfaces increase under the same law as that by which the force of gravity diminishes. No matter, then, what the difference in diameter, the above principle holds good, and the attractions of a central particle upon every possible spherical shell of unit density must be equal. Of course the same principle will apply to all similar and proportional portions of such surfaces, though the one contain but a single molecule, and the other myriads of molecules.

These surfaces must be infinitely thin for an exact application of the law, and the existing equality is that of possible, not of actual, attractions. It could become actual only in the case of the two shells being exactly equal and uniform in density. Gravitative attraction is not a positive, but a relative vigour. Each atom pulls upon every other atom with an energy dependent upon the distance and the reverse attractive energy of this atom. Here, as in every case, action and reaction must be equal, and the total attractive energy exerted by the atom must be just equal to the total attraction exerted upon it by all the other attracting atoms in the universe. Its total energy is, therefore, subject to extreme variations. If an atom be removed from the sun's centre to a position in space midway between the Sun and Sirius, its total attractive energy would be greatly decreased.

The principle of equal attraction on equal spherical shells of unit density will enable us better to appreciate this principle of loss and gain. The total attraction of a central particle must depend on the total number of such shells into which all other particles can be formed. The possible number of these is largely influenced by the location of the particle. For the quantity of matter necessary to compose such a unit shell increases with distance in the ratio of the square of its radius. In contiguous space a very small quantity of matter would form a unit shell. In remote space a very great quantity of matter would be requisite. Consequently the possible number of such shells depends upon the greater or less contiguity of matter as a whole. If the average distance of all matter be greater in one case than in another, then each unit shell will consume more matter, and the possible number of such shells decrease.

The total attraction of the central particle will decrease in proportion. A particle in the centre of the sun is surrounded by dense contiguous matter, one in open space by rare contiguous matter, while the more distant matter of the universe does not greatly differ in its relations of distance to the two particles. Therefore the first is subject to a much greater total attraction than the second, and its reverse energy of attraction is much greater. The attractive energy of a particle, then, is far from being a constant quantity, but varies with every variation in the relations of distance of surrounding matter.

These variations in attractive energy appear to make themselves instantaneously felt by all matter, there being no indication that time is necessary for the movement of this energy. We may not be able to comprehend the cause of this instantaneous action, but we may draw a certain deduction concerning it. For if the central particle has an equal attraction upon every possible shell of surrounding matter of equal density, it follows that it has an attraction upon every existing shell in proportion to its density, and upon every segment of such a shell in proportion to the density of this segment. Thus the attraction of a central upon an exterior particle is governed by the following rule: —Supposing that the attraction of the central particle upon the whole surface of a shell of unit density be taken as a unit of attraction,—if, then, any exterior particle be reduced to unit density and thickness,—it will form a certain fraction of the whole area of a unit shell, and will be attracted by that fraction of the unit attraction.

Let us suppose lines to be drawn from the central particle to every portion of the edge of the exterior particle thus reduced. These lines will compose a solid angle. If now the exterior particle be moved so as to form part of the surface of a shell of half the diameter of the preceding, and a solid angle again formed, this angle will be four times greater than the preceding one. Instead of saying that the attraction is increased fourfold, we may say that the angle or the leverage of attraction is so increased.

We may then affirm that the vigour of attraction depends upon the degree of leverage exercised, masses of unit thickness and density, which form the same solid angle at an attracting centre, being equally attracted, whatever their distance from that centre, or their relative sizes. If any mass approach the centre the angle increases, if it recedes from the centre the angle decreases, and the leverage of attraction increases or decreases in accordance. If a particle

fall towards another it is scarcely correct to say that the vigour of attraction constantly augments, with the idea of an instantaneous projection of increased increments of attraction from one particle to the other. It seems more correct to say that the effectiveness of a fixed and constant vigour of attraction augments as its leverage increases, or as the solid angle subtended by the falling particle increases in dimensions. The central particle seems to send out lines of attraction which extend throughout space, with equality of possible energy at every distance. The number of these lines cut by a mass of unit thickness determines its gravitation towards the particle. If it approach the particle it cuts an increased number of lines; if it recede it cuts a diminished number; and the effective attraction increases or diminishes accordingly. If, finally, the attracted mass exceed the unit thickness, it may be viewed as a series of separate unit masses, each feeling a separate vigour of attraction. If it vary from the unit density the attractive vigour also varies in accordance, there being more or fewer of the lines of possible attraction cut by the matter of the mass. This idea of lines of attraction is given but as an illustration. There is no real attraction except where matter is encountered, and such a line encountering a small quantity of matter would feel a far less vigorous attraction than one which encountered a great quantity of matter. The acting particle is, of course, forced to move in the direction in which the effective energy of its lines of attraction is greatest.

Leaving this abstract deduction from the principle of Gravitation, we may proceed to review certain concrete results of its action, in connection with the action of other physical forces. Returning again to the question of terrestrial gravity, it may be well to consider the results likely to arise during the condensation of a nebular mass into the present condition of the earth, through the action of gravity. In this process gravity has one opposing energy to overcome —that of heat. And the final result must depend on the inter-relations of attraction and of heat dissemination.

If the density of the original nebula approached uniformity, then the greatest energy of attraction towards the centre must have been felt at the surface. The process of condensation, however, would soon produce an interior compression, the region of greatest weight per unit volume extending inwardly. But this compression would be resisted by the generally distributed heat of the interior matter. And as the centre was approached condensation would be less and less assisted by gravity as a direct agent, since

gravity must decrease in vigour downwards, and become null at the centre.

The heat contents of such a nebular mass may be considered as absolute heat, the temperature being to a certain degree uniform. But the effect of gravitative condensation, either directly through attraction or indirectly through compression, necessarily rendered some of the latent heat sensible, and thus increased the temperature of the condensed regions of the nebula. The resistance to compression would be the same whether this heat existed as sensible or as latent heat; but as soon as it became sensible the disturbance of temperature equilibrium introduced a new element into the case. The heat commenced to flow from the denser to the rarer portions of the nebula, thus tending to reproduce temperature equilibrium, but to overthrow the original heat equilibrium. As compression, with its disturbance of temperature, must have first made itself felt near the surface, the heat yielded would act to hinder its extension downward. This heat must be first got rid of ere compression could effectively descend. But the getting rid of such heat would be a very slow process. The principal agency in this process would be heat convection, or the movement of the heated matter. There may have been radiation also, if the nebulous matter was transparent.

Such a loss of heat must have been most rapid at the surface, where gravity was greatest and radiation most free to act. The chilled surface matter, becoming heavy, would sink; the heated internal matter, becoming light, would rise. Thus there would be a double convection, that of heated matter upwards and of chilled matter downwards, and heat would be got rid of by both processes, being radiated from the surface, and also employed in re-heating the chilled matter. By a continuation of these processes the compressing effects of gravity must have made themselves more and more deeply felt, the resisting heat being gradually disposed of. Had the acting matter continued in the gaseous condition the above process might have continued until compression was fully produced internally, and there was a regular increase of density downward to the centre.

But long ere this result could have been reached surface solidification very probably took place, with the result of cutting off the distribution of heat by convection and radiation from the interior, and leaving only the interminably slow process of conduction. Therefore if the full effect were not produced before this solidification took place, it would be interminably delayed. And another delaying influence

would be the following :—If the compression of gravity first condensed the region of matter adjoining the surface, and yielded sensible heat, which heat was distributed to regions containing less sensible heat, this would take place as well towards the region of lower temperature at the centre as towards that at the surface. As a necessary consequence the central region must have grown hotter instead of cooler, and rarer instead of denser. And long ere this effect could have been reversed by the gradual approach of condensation to the centre, the solidification of the surface was probably produced, and the ready outflow of heat prevented.

The argument often advanced of late, that as solid matter was formed it would sink to the centre, and thus gradually produce a solidified core, does not seem well founded. The descent of this solid matter would be opposed by friction with the interior gas, while its weight would decrease as it descended. The decreased pressure and increased temperature to which it was exposed interiorly would tend to re-vapourise it. Long ere this process could have carried condensation to any extreme depth the local formation of solid matter at the surface would most probably have become general, and a continuous shell of solid matter have embraced the earth. The rapid radiation from the surface undoubtedly tended to produce this effect long ere the full results of compression could have been felt interiorly. In the present condition of the sun we have an illustration of this earlier stage of terrestrial evolution. In the solar orb the sensible heat yielded by the growing compression of the interior substance comes to the surface by a rapid convection, and is rapidly radiated outwards. Reversely, the dark spots seem regions of downrush of the condensed surface substance. Such a process must continue long after the surface condensation becomes sufficient to produce local solidity. Yet as interior compression becomes greater the convective action must become less rapid, and the outflow of sensible heat less active. Also increased compression must decrease the facility of descent of solid matter. Consequently the surface condensation must grow more declared as it is less hindered by upflowing heat, and solidification in time become general.

This effect must eventually be produced in the sun. It has, since a remote period, been produced in the earth. When once the solid shell had attained a certain thickness and rigidity it must have become permanent, and the outflow of internal heat have been, in great measure, confined to the slow process of conduction through this solid material. As the solid surface became thicker and more rigid its

pressure on the interior gas would be reduced, and might, in time, come to just equal the resistance of this condensed gas. That point once reached, compression by weight of the solid surface would not continue. The gaseous interior might yet contain considerable volumes of sensible heat, the result of previous compression. If this existed in regions intermediate between surface and centre it must move in both directions. It would be conducted upwards, and escape by radiation into space. It would be convected downward, and tend to increase the rarity of the central gas. There must, in fact, be a constant effort to produce uniformity of interior temperature.

The gradual loss of heat by the layer of gas in contact with the interior solid surface might act to produce liquefaction or solidification of this layer, and the thickness of the crust be thus increased. But such a process could be but temporary, since it would tend to rarify the remaining gaseous contents of the earth's interior. Such a rarefaction would act to convert sensible into latent heat. Hence the solidification of a portion of the gas would necessarily reduce the temperature of the remainder, and this might continue until the general temperature of the interior gas and the solid shell became uniform. This condition attained, there could be no further solidification of the gas, except as a result of a gradual cooling of the whole system. The refrigeration of the gas produced by this loss of sensible heat would not affect its physical condition, since its absolute heat contents would remain the same as before.

Yet the fact that a rigid solid shell was formed would not necessarily hinder a continuance of processes in the interior similar to those already considered. The globe of interior gas would in some respects resemble the original globe of nebulous matter, and gravitative compression might go on in it independently of the fact that it is surrounded by a solid globular shell. But the special conditions surrounding this gaseous globe would be markedly different from those surrounding the original nebula. The free radiation from the surface of the latter would be here checked, and reduced to a slow conduction through the solid shell. Thus the heat product of compression could be but slowly disposed of, and condensation could proceed only very slowly. It may be supposed, however, that it would continue until gravity had produced its full effect, and the gas had become regularly denser downward to the centre. But it must be borne in mind that the action of gravity here would also be greatly modified.

The effect of gravity on the surface matter of the original nebula was almost wholly centripetal, the material of surrounding space being too rare to strongly attract it. If now we take the conceivable case that the original nebula was at one time of practically uniform density throughout, the centripetal gravity of the matter of this nebula would necessarily decrease regularly from surface to centre, there being a reverse attraction upwardly at every point below the surface. The first effects of compression would necessarily be superficial, and would at once modify this uniform decrease of gravity. The condensed matter would grow heavier in proportion to its density, its increased downward pressure overbalancing the upward attraction. Thus the first effect of compression would be to remove the region of greatest centripetal gravity to that point in the interior at which the increased weight produced by condensation was just balanced by the increased effectiveness of the centrifugal gravity. This point would descend more and more deeply as condensation descended and grew more effective. But the disturbance of the original uniformity of gravity thus produced would be accompanied by a disturbance of an opposite character. As the gravity at any internal point must be determined by the vigour of centripetal gravity diminished by the vigour of centrifugal gravity, it follows that, as the surface layer became condensed, the centrifugal gravity increased in vigour. Thus the layer of rare matter adjoining that of dense matter had its centripetal gravity, or its weight, decreased. And this disturbance must have made itself felt, with diminishing effect, to the centre. If we take a portion of matter anywhere in the interior, this would experience two increments of attraction,— one from the matter laying between it and the nearest surface, the other from the matter in the line passing through the centre to the farthest surface. While all this matter was uniform in density, the vigour of these two lines of attraction would depend strictly upon their length. But when the matter at the two ends of the line— or the two surface regions—became condensed, these two portions of the line would exert increased attractions, in proportion to their density. But, under the law of diminution of attraction with distance, this increased attraction of the near surface would considerably overbalance that of the distant surface. Therefore, with every increment of surface compression, the downward attraction of every portion of the rare interior matter must have decreased, this decrease making itself felt to the centre.

Pursuing this thought we can readily conceive of a marked

result of such a process. One of its effects would be to diminish the rapidity of internal compression, so that the surface region may have become greatly condensed, or even solidified, to a considerable depth, while the reduction of gravity and the resistance of temperature kept the interior substance in the state of a rare gas. But as the superficial region grew denser the centrifugal gravity of the portion of gas adjoining it internally must have augmented, until in time it may have equalled the centripetal gravity. We would have the case, in fact, of a mass of matter in contact, on one side, with a thick region of dense matter strongly attracting it upwardly; and, on the other side, with a much thicker region of rare matter, attracting it downwardly with comparative weakness. And this rare region would be complemented, at a very considerable distance, with a dense region equal in thickness to the first, but so much more distant as to greatly reduce its attractive effect upon the mass of matter in question.

In consequence, it is not difficult to conceive of a process of terrestrial evolution somewhat as follows:—Compression of the nebula necessarily first took place at the surface. As it extended inwardly the heat set free hindered its rapid extension. The attraction of this dense surface region upon the interior gas reduced the centripetal gravity of the latter, and possibly, at length, completely negatived it. It may, indeed, have gone further than this. It may have reversed the gravity of the matter adjoining the inner surface of the dense surface shell. Instead of gravitating downwards, the matter in this region may have commenced to gravitate upwards, the effective attraction of the adjacent dense matter overbalancing that of the interior rare matter and the distant dense matter of the opposite surface.

In such a not inconceivable condition of things there would be a most effective hindrance to the inward extension of density. This, at first hindered by the resistance of temperature, became also hindered by a growing reduction of the downward gravity of the interior substance. Eventually this downward gravity may have become first negatived, and then reversed, it being replaced by an upward gravity. Going downward from the surface we would first pass through a region of downward pressure. At a certain depth this would reach its limit of effectiveness, the extra weight produced by increased density being balanced by the reverse effect of upward attraction. Going still deeper, the effect of this upward attraction must augment, and the downward gravity decrease. This decrease must regularly continue

until, at a considerable depth, the two increments of attraction may become equal, and a region—or a spherical shell—of no effective gravity be reached. Beyond this region the gravity may possibly become reversed, and the pressure of matter be upwards, instead of downwards. Going still deeper, and entering the gaseous region, this reversing influence would gradually diminish with descent, as the neighbouring dense matter was receded from, and the distant dense matter approached. Finally, at the centre, another region of no gravity would be found, the opposite attractions being again balanced.

Thus there would be a surface shell of downward gravity, at first increasing, and then decreasing to an interior limiting shell of no effective gravity; beyond this a deeper region of upward gravity, which again, after reaching its limit of effectiveness, slowly decreases, and merges in a second region of no effective gravity at the centre. Of course this hypothesis rests on the assumption that the earth consists of a thick shell of solid matter surrounding a, perhaps much thicker, gaseous interior. In such a case the deductions above presented seem beyond question. And that such is the interior constitution of the earth the necessary behaviour of heat in a condensing nebula certainly renders probable.

If condensation was confined to the superficial region until considerable density was produced, there would certainly be a reduction of the centripetal gravity interiorly. And if the superficial condensation reached a certain degree ere making its effect felt in the deep interior, then the gravity of the rare material adjacent to the inner dense surface must necessarily be so reduced by upward attraction as to be finally negatived. But compression would not cease with the production of this region of no effective gravity. Although no longer aided by gravity, the pressure of the dense material above must still have made itself felt, and produced condensation below this point. This would tend to produce the reversal of gravity above considered, and gravitative compression would now be upward instead of downward. Its tendency, therefore, would be not to augment, but to decrease the centripetal downward compression of the interior gas, and there must result a compression proceeding from centre to surface, not from surface to centre. The gas would become densest at the region adjoining the solid crust, and rarest at the centre. But such a process could not indefinitely continue. The growing rarity of the remaining gas as a portion of it became condensed, and its decreasing temperature in consequence, must necessarily act as a limiting check to condensation, and yield a final equilibrium.

In such a manner the earth may have reached, or may eventually reach, its final state of condensation, this increasing downward to a certain distance from the surface, and thence decreasing to the centre, and being at every point in accordance with the effective gravitation, the pressure, and the physical state of its material at that point.

The above considerations lead to some thoughts on the constitution of atoms. It is certainly not unreasonable to conclude that in the formation of atoms the same laws of fluid aggregation are obeyed as display themselves in the formation of suns and planets. There can be no question of great and small in the operation of natural laws, though differences in physical condition between great and small might produce certain differences in results. Thus the effects produced by solidification in great masses would not display themselves in minute masses. So the heterogeneity of great nebulous masses might be replaced by homogeneity in minute masses. These differences might produce differences in results, but under like conditions the law of aggregation must act uniformly, without distinction of great or small.

In a completely homogeneous nebulous mass condensation must proceed uniformly; there could be no minor centres of aggregation, and no rotation of the mass, since every tendency to produce rotation in any one direction must be counteracted by an equal opposite tendency. In a heterogeneous mass minor centres of aggregation must display themselves, and the tendency to rotate in certain directions must overbalance that in others. It may be well to remark here, also, that all these processes would proceed simultaneously, so that the formation of the various bodies of the Solar System—as considered in my paper in this Journal for May, 1881—must have been simultaneous, instead of successive, as commonly considered.

The secondary centres of gravity in the original solar nebula affected gas volumes of the greatest diversity in size, from the planet down to the atom. The subordination of each such aggregate to the greater aggregate of which it formed part was an external, not an internal one, and it is very probable that the same principles of aggregation affected alike the sun and the atom.

We may imagine several modes of atom formation. In the case of its arising through the centripetal attraction of a mass of matter, this matter might be homogeneous and might be heterogeneous in density. In the former case all

the matter would condense directly upon the centre, and no rotation could arise. In the latter case the centre of gravity affecting each particle would not be the centre of mass. There would be motion tangential to the centre, and stronger on one side than on the other, and rotation must arise. This might develop two species of atoms, a rotating and a non-rotating. But the latter, most probably, could not retain their aggregation, since they could present no elastic resistance to the impacts to which all atoms are constantly subjected, and they must therefore be exposed to disintegrating forces.

But there is still another mode in which atom aggregation may have taken place, and this perhaps the most effective, if not the only, mode. In short, each atom may be a minute eddy, whirlwind, or cyclone in the ether. In the motions of the atmosphere it frequently happens that two opposing currents of air meet. Unless in the improbable contingency of this opposition being exact, their effort to pass each other results in a rotation, of which we have a familiar instance in the small whirls of air which so frequently arise. Similar phenomena are common in water, the meeting of any two diverse currents producing a revolving eddy. In fact, this result of the movement of fluids is a very common manifestation, and varies from the great whirl of the cyclone to the minute whirl of the air or water eddy. But the ether is a fluid affected by incessant motions. Minute winds of ethereal matter may be constantly moving in every possible direction. Such winds must constantly meet each other, and the inevitable result must be an eddy, or minute revolution of this fine fluid. But the eddy here may be ordinarily of such minute radius that the general centripetal attraction of the moving substance may suffice to retain its organisation. In short, a revolving mass thus formed may persist in this condition through the influence of the mutual attractions of its substance. It would compose a permanent atom.

If, then, we consider an atom as a mass of disintegrated matter, condensed about its centre of gravity, and rotating about a central axis, it would closely resemble the general conditions existing in a planet. But aggregation could not proceed so far as in the planet. The mass must continue gaseous. And the relations between its rapidity of rotation and its vigour of condensation might be far different from those existing in the planet. If the centrifugal energy of rotation was great as compared with the centripetal energy of condensation, the mass might assume a discoid shape,

the most effective energy of gravity being in the line of the axis. The same results must arise as in the case of a planet. The region of greatest gravity would be originally at the surface, but would extend inwardly as compression was produced. But at a certain distance inward a limiting region would be reached at which upward and downward attraction was balanced, and the mass would grow more rare from that region to the centre. Also the centrifugal energy of rotation, by its resistance to gravity, would tend to bring the region of greatest compression nearer the surface. In short, if the energy of rotation was considerable as compared with that of condensation, the original globular mass might become a flattened disk of condensed matter. From the surface region of greatest condensation matter would become more and more rare to the centre, and also more and more rare outwardly, shading off into the exterior disintegrated matter.

There could be no sharp lines of demarcation as between the solid and gaseous surfaces of the earth; but the atom would be represented by a gaseous earth, with a region of greatest compression, and a rarity increasing regularly to the centre and regularly outward to the atmospheric limit. Its vigour of rotation must also decrease outwardly, and gradually shade off into the motion of free exterior matter, as in a former paper I have supposed the atmosphere of the earth to do. In short, I suppose an atom to be an exact analogue of a gaseous earth in which the utmost possible effects of gravitation have been produced, whose substance shades off into, and is practically continuous with, that of interspace, and whose motion of rotation gradually decreases outwardly through friction, as the energy of the centripetal attraction is reduced by distance, until finally all motion of rotation is lost. The atom would differ from the earth principally in having no inter-condensation of its material, it being an aggregation of disintegrated instead of integrated matter.

Assuming that what is usually called Ether is simply disintegrated matter, the atom would be a rotating aggregate of condensed ether, with an atmosphere shading off into the rare ether. Between atoms, as between spheres, there would be a region of free ether. And the possible quantity of atom formation must be governed by the same laws that limit spheral aggregation. For the condensation of ether into atoms must yield sensible heat on the same principle as in the condensation of atoms into spheres. This condensation of a portion of the ether must also rarify the remainder, and

reduce some of its sensible heat to the latent state. Temperature equilibrium would be disturbed, and could only be restored by the outflow of sensible heat from the atom to the ether. As a result the density of the former would increase, and that of the latter diminish. This action might be almost instantaneous, but its effects must be the same as in the case of larger bodies; it must limit the degree of atom formation. As atoms were formed the adjacent ether must have grown rarer, and its absolute heat have increased, until its centrifugal exceeded its centripetal energies. This relation between atoms and ether once attained, no more atomic aggregation could take place.

As atoms aggregated into molecules, and these into spheres, the ether between them must have been forced out, and have flowed into interspace. But the vast volumes of sensible heat produced by this secondary condensation also flowed into interspace. Through its action the centrifugal energy of interstellar matter must be increasing, and a reverse process of disintegration taking place there, atoms being reduced to the ethereal state until the necessary relations of equality between centripetal and centrifugal energy are reproduced.

Matter is practically continuous; but it cannot be really continuous unless ether have elastic properties enabling it to swell and fill vacancies. Except in this improbable contingency there must be voids left as matter condenses. But these voids are practically occupied by moving matter, the centrifugal energy of ether, as well as of atoms, keeping it in constant activity, so that no such void could remain empty more than an immeasurably small interval of time. The vibrations of light which pass through ether may pass through atoms and molecules as masses of ether. If accordant with the pitch of the atom or the molecule, they are absorbed. If accordant with the pitch of the atomic ether, they are transmitted. If discordant with either, they are repelled.

As we have spoken of the elasticity of a rotating atom, and the lack of elasticity in a non-rotating atom, it may be well to close with a brief consideration of this subject. Rotating atoms might be perfectly elastic. The particles would retain their lines of movement around the centre with considerable vigour. If driven from these lines, all the energy of the central force would aid in regaining them. The side of an atom, for instance, might bend deeply towards the centre under the influence of a blow, but the curve-preserving energy would force it out again, and thus

repel the striking substance with a vigour equal to that of the blow. This may be the secret of all elasticity—this forcible preservation of lines of movement under the influence of the attraction to which they are due. The elasticity of liquids may be due to movements of rotation; that of solids may arise from the forcible preservation of their lines of vibrating movement. The more rigidly these are retained, the more elastic the solid. But when the particles are not held with an attractive vigour sufficient to draw them back to the points from which they are driven, the solid becomes inelastic. When particles freely assume new lines of movement, elasticity ceases.

II. MINIATURE PHYSICAL GEOLOGY.

By C. Lloyd Morgan, F.G.S., Assoc. R.S.M.

(Concluded from page 640.)

VIII. *The Cutting Backwards of Rivers.*

THE ordinary typical river forms and deepens its valley by a combination of two processes, cutting downwards and cutting backwards. It is interesting to note how closely the two methods are connected. At first sight, indeed, it would appear that most valleys are almost entirely produced by the river cutting downwards. But a little further thought enables us to see that this very action is produced by the *fall* of the river, for if there be no fall the stream ceases to flow; and it is this fall of water that causes the cutting backwards. It is convenient, however, to keep the term 'cutting backwards' for the case of a river forming its valley or ravine by means of a waterfall.

The most notable example of this on a large scale is, of course, the formation of the Niagara ravine, some 7 miles long and about 166 feet in height, which stretches from Queenstown to Niagara Falls. The water of the Niagara River falls over a ledge of limestone. Below this limestone is a shale which—being easily decomposed and readily acted upon by the spray that is continually rising from the Falls, aided, I have no doubt, to a great extent by the action of frost—is rapidly undermined, until huge blocks of the lime-

stone above, being no longer supported, break off and dash to the base of the Falls. It is partly owing to the fact that the shale is undermined in this way that it is possible to walk for some distance under the Falls. When I was at Niagara it was not possible to go very far, but the scene was sufficiently striking. It was winter, and all the edge of the waterfall was fast locked in the icy grip of frost. On looking up, therefore, I saw hanging above me festoons of icicles, some of them a hundred feet long. With difficulty—for the rock was glazed with ice—I proceeded still under the Falls, and then, on looking up, I saw that which I shall never forget: the whole river took its mighty leap, and fell with a bewildering roar at my very feet. Though almost blinded by the spray, which froze to my hair and beard, I could not take my eyes from the curving green mass of water which shot over my head. In ten minutes I learnt a lesson concerning the power of water that the reading of a lifetime would not have taught me.

Of this cutting backwards, which in the case of Niagara is at the average rate of nearly a yard per annum, it is not difficult to find miniature examples. In a note-book, for instance, I find the following:—"Near Reculver, in Kent, a small stream is led through pipes from the Coast-guard Station to the cliff. This stream has formed a little valley by cutting backwards. Between a ploughed field and the edge of a cliff there is a band of about 2 yards of grass. The little valley formed by this stream has cut its way backwards through the grass, and for a distance of some 6 or 8 yards into the ploughed field. The whole has evidently been effected so recently that it forms an instructive example of geological action on a small scale." Below the waterfall, again, at Black Gang Chine, Isle of Wight, the little stream falling from a thick ledge of hard sandstone, over softer, more friable sandstone, affords a good small scale illustration of the action of the water at Niagara. On the way up Table Mountain, once more, above Bishop's Court, when the first ridge or terrace is reached, the path passes a miniature example of similar action, the place of the upper ledge of limestone being in this case taken by matted grass.

IX. *The Formation of Deltas.*

On the Ramsgate sands many of the miniature rivers flow into pools which are miniature seas or lakes. "I have often seen one of the streams in the course of an hour fill up a considerable bay, and push its delta far out to sea. The

grains of sand, when they come to rest in the pool, form a slope of very constant angle, which by a number of measurements I found to be 40° for coarse sand and 34° for fine sand, the average angle being 36°."—("Nature," March, 1878.) I have repeated these measurements on miniature deltas on the Cape Flats, and found similar angles. One of my notes on these miniature deltas is to the following effect:—"The delta seems to push further and further out into the miniature sea, until at last it has formed a long low spit. If now a little freshet comes down, the stream overflows to some extent its own delta, and ere long forms at one side of its original course a fresh channel, which leads to the sea by a shorter course." I was not aware at the time that I wrote this note that a similar action occurs on a large scale. In Mr. Belt's interesting "Naturalist in Nicaragua," however, I find the following passage:—"If the Colorado were not to be interfered with by man, it would, in the course of ages, carry down great quantities of mud, sand, and trunks of trees, and gradually form sand-banks at its mouth, pushing out the delta further and further at this point, until it was greatly in advance of the rest of the coast; the river would then break through again by some nearer channel, and the Colorado would be silted up, as the Lower San Juan is being at present. The numerous half-filled-up channels and lagoons throughout the delta show the various courses the river has at different times taken."

X. *Miniature Drift Currents.*

Below the chalk cliffs between Ramsgate and Margate there are, at low water, long pools, in which I have sometimes watched with interest the formation of miniature drift currents. "I remember watching with interest such a current, which flowed between tiny chalk cliffs through the straits which separated two miniature seas; the most instructive point being that the finer grains of sand at the bottom of the straits, where the water was some 7 inches deep, were rolling over each other in such a manner as to prove the existence of an under current setting in *the opposite direction* to that in which the surface current was flowing."—("Nature.") On a subsequent occasion I was able, by means of more or less loaded bladders, to see one near the top drift in one direction, another near the bottom drift in the opposite direction, while the third, sunk half-way, remained nearly stationary. At the leeward end of this pool there was a miniature delta, upon which the waves gradually

advanced, planing off the upper portion, and forming tiny cliffs of delta material, but leaving the deeper parts of the deposit intact.

XI. *Miniature Tides.*

When a little stream flows into the sea one may watch, as the waves roll in, estuary tides in miniature. The point with respect to these to which I directed my attention was, whether the tide as it came up "made" first along the shore or in mid-stream. But though it always did one or the other, and seemed always to do the same in the same stream, I am unable to say which is the more general. Nor can I say what causes bring about the one result or the other. I fancy that where the stream has shelving banks, and runs in a concentrated current, the tide makes alongshore; but that where the stream is wide, and widens out still more where it enters the sea, the tide makes up the main channel. This view, however, I cannot substantiate by observation.

XII. *Miniature Lake Terraces.*

On the sands near St. Catherine's Point, in the Isle of Wight, and again not far from Dolgelly, in North Wales, I have noticed the formation of miniature lake terraces. The only point worthy of notice with regard to these is, that they never extended individually more than half a foot or a little more in length, and that the terraces were at various heights. It seemed as if the formation of terraces was more or less continuous if the whole lake was considered, but intermittent if particular portions of the lake were regarded. This seems to me to help us to understand how a *series* of terraces, such as those which are found on the shores of Lake Superior, may be made by a *continuous* lowering of the waters of the lakes.

XIII. *Sand Ripples.*

On any extensive stretch of sands the way in which the sand-ripples are formed by the wind may be watched. It is curious to notice that a series of ridges is almost invariably formed. These ridges present a sloping face to windward, and an abrupt face to leeward. When it is blowing hard a quite remarkable sand haze may be seen quivering over the surface, as lighter sand grains are caught up by the wind;

and after bathing I have noticed a stinging about the ankles produced by the cannonade of little grains, the sand meanwhile being heaped up on the windward side of my feet. During a gale, and still more during continuous winds such as the 'south-easters' of the Cape, this sand cannonade must have some effect in eroding even hard rocks. That this actually *is* the case may be very clearly seen on the Cape Town side of Camps Bay, where the granite rocks have a very peculiar surface, eaten into deepish conical holes, which I feel sure have been produced in this way.

The sand-ridges above mentioned are constantly advancing in the direction in which the wind is blowing, but they are stopped by the smallest stream. I was struck some years ago, when I was in Cornwall, at the fact that quite a small stream stopped the advance of the sand dunes near Hayle, so that this stream formed a boundary between green grass flats and a wilderness of sand. This reminds one of Prof. Jukes's remark:—"Egypt would probably have been long ago obliterated by drift sand, if it had not been for the Nile, and the strip of vegetation that accompanies and defends it."

Near Ouchy, on the Lake of Geneva, I had a good opportunity of seeing the formation of sand-ripples under water. My note on the subject runs thus:—"As each wave passed the little ridges advanced, the sand being raised in a small cloud up the sloping side of one ridge, over the intervening gap (falling down the steeper side), and being deposited on the sloping side of the next ridge. Thus the whole set of ridges advanced gradually."

XIV. *Miniature Earth Pillars.*

Those who have visited Switzerland or the Tyrol probably know well the nature of earth pillars. Those who have not perhaps know something about them at second-hand, through the pages of their Lyell. Such earth pillars may be seen in miniature almost anywhere. On the sea-shore a broken shell protecting the sand beneath it from the action of rain becomes raised on a little pedicel or miniature earth pillar. The sloping banks of the very ditches offer examples sometimes 2 or 3 inches high: these are, however, so well known as to call for no comment here. I may only mention that the finest miniature examples I have seen were on the Tijuca road, not many miles from Rio. They were in a decomposed granite slope, were capped by bits of less decomposed rock, and were about 9 or 10 inches in height.

XV. *Miniature Mountain Sculpture.*

On the south side of Hout's Bay, in the Cape Peninsula, one of the many little 'kloofs' scooped out in the granite shows at its head a curiously cut face of the rock, which is here easily decomposed. The head widens out to about 50 or 60 yards, and forms a sloping cliff of some 30 feet in height, at the top of which are oddly-perched boulders of much harder granite. This forms the upper end of the kloof, and is evidently working backwards year by year. It seemed to me to afford a miniature illustration of the carving out of a mountain face. In the granite the water has cut out a number of ravines, some broader, some narrower, which seemed only to want vegetation to make them perfect miniature kloofs. Between these projected buttresses, of perhaps somewhat harder rock, one of these ridges, when viewed from above, was seen to have a well-marked succession of shoulders; here and there were vertical faces ending in a steep slope. From the nature of the rock there were no signs of the terraces which form so marked a feature of the Cape Peninsula mountains, composed as they are of more or less even-bedded sandstone. Near the foot of the face there was one very marked rounded spur, running out at right angles to the head of the kloof. In fact, as I looked from the granite face, cut out as I have very briefly described, to the steep mountain slopes which here border Hout's Bay, I could not but be forcibly struck with the similarity of the two. And this steep face, be it remembered, has been formed by the action of atmospheric agencies and stream wash from a sloping hill face.

XVI. *Recent Fossilisation.*

Perhaps I may here be allowed to draw attention to the process of fossilisation which may almost be seen in progress in the blown sand of the opposite side of Mount's Bay, as well as near Kalk Bay and elsewhere, in the Cape Peninsula. This blown sand consists partly of sand grains, partly of the comminuted fragments of shells. In process of time it covers up such scrubby vegetation as lies in its path. Shut out from the light of day and from the air, this dies, and begins to decompose. But as each particle of woody fibre is removed, its place is taken by a particle of lime, taken up from an aqueous solution of the shell fragments. Thus bit by bit, molecule by molecule, the wood is

replaced by limestone, which preserves, in a state of greater or less perfection, the form of the decaying root or branch, and becomes a complete pseudomorph. This has gone on to such an extent that the sand is here and there strewn with these pseudomorphs, while it is not difficult to find calcified roots *in situ*, even the most delicate filaments being perfectly replaced by brittle limestone.

XVII. *Conclusion.*

I have set down these miniature observations in the belief that, trivial as they seem, they have some *educational* value. I am firmly persuaded that they and their like educe and develop faculties of observation, and thus throw the mind into a fit attitude for observing on a larger scale should opportunity occur.

III. THE POISONOUS POWER OF METALS.

M. CH. RICHET has been experimenting on the comparative poisonous power of different metals, by observing what quantity of each may be dissolved in a given volume of sea-water without proving fatal to fishes immersed therein in less than forty-eight hours. The maximum dose of each metal in the state of chloride, compatible with life for the time specified, he names the "limit of toxicity." The species of fish experimented upon were *Serranus Cabrilla*, *Crenolabrus Mediterraneus*, *Iulis vulgaris*, and *I. Geoffredi*. A previous set of experiments had shown that these fishes were capable of surviving for an indefinite time—*i. e.*, longer than eight days—in 2 litres of sea-water, placed in a wide crystallising pan, open to the air. M. Richet seems to have proceeded in the following manner:—Dissolving in the water a large excess of the metallic salt in question, and, noting how long the fish survived, he gradually reduced the dose till he reached a quantity which did not prove fatal. In all cases chlorides were used, since a special trial had shown that they were less poisonous than the corresponding nitrates.

The sulphates he rejected as being too sparingly soluble for such experiments. In this manner he worked through mercury, copper, zinc, iron, cadmium, ammonium, potassium, nickel, cobalt, lithium, manganese, barium, magnesium, strontium, calcium, and sodium, and found their toxicity decrease in the order here given. Thus whilst the largest quantity of mercury in the state of perchloride, compatible with life, was 0·00029 grm. per litre, in sodium the limit rises to 24·17 grms. in the same volume of water. Several of the most poisonous metals, such as arsenic, antimony, and lead, have not been experimented with.

The author calls attention to the fact that, according to his results, there is no definite relation between the atomic weight of a metal and its toxicity. Copper, according to his table, is 600 times more poisonous than strontium, though its atomic weight is less. Lithium, whose atomic weight is only one-twentieth that of barium, is still three times more poisonous. Even in metals of the same chemical group the relation between atomic weight and toxicity does not appear. Thus cadmium (atom. weight 112) is by one-half less poisonous than zinc (atom. weight 65). Lithium (17) is seventy times more poisonous than sodium (23).

The first point which strikes us in examining the results of M. Richet is their want of agreement with the conclusions reached by Dr. James Blake ("Journal of Science," 1881, p. 318). This experimentalist found that, as regards bodies belonging to one and the same isomorphous group, the higher the atomic weight the more intense is the physiological action. M. Richet finds no such relation, as may be seen from the last two instances given above. He declares also that there is no relation between the chemical function of a body and its toxic power, giving as an instance the experimental result that potassium is 250 times more poisonous than sodium.

But it would be, we think, premature to pronounce any decided opinion concerning these and other discrepancies which might be particularised. The methods of experimentation in the two cases are totally different. M. Richet, as we have said above, immersed his subjects in poisonous solutions, and simply noted the facts of death or survival, and the time in which the treatment proved fatal, if at all. Dr. Blake injected known doses into a vein or artery, and recorded not merely the fatal quantity, but the nature of the symptoms produced, and noted the curves described by a kymograph connected with the femoral artery. His researches, too, have extended to forty-one of the metals, and

have been continued for forty years. A point of difference not to be forgotten is that Dr. Blake operated exclusively upon mammals and birds, for which M. Richet's method would have been impracticable. The latter inquirer considers that by his immersion method "we escape the inconveniences due to the variable weight of the animal, and to the disturbing chemical reactions which follow on a sudden injection into the blood." We are by no means sure of such advantages. Without especial experiment—of which M. Richet makes no mention—it may be considered at least an open question whether a smaller subject might not succumb to a weaker poisonous solution than would a larger animal of the same species. It is also more than probable that the absorption of the various metallic salts into the circulatory system through the skin, and especially the gills, will vary quite apart from the poisonous power of each.

In the cases of iron, strontium, and barium, the author tells us that the sulphates and phosphates present in the sea-water were previously eliminated by means of a suitable quantity of barium chloride. This operation, he maintains, did not appreciably affect the vital conditions of the fishes submitted to the experiment. On this point we must plead guilty to a slight degree of scepticism.

The results obtained by M. Richet differ also from those obtained in general experience on the action of poisons introduced into the animal system by the alimentary canal. Thus the soluble salts of barium are, when swallowed, very powerful poisons. In M. Richet's scheme they rank as less formidable than the compounds of iron and potassium. Everyone knows that a method has been proposed for purifying "plastered" wines, by treating them with a quantity of barium tartrate proportionate to the sulphuric acid fraudulently introduced. The suggestion, theoretically very happy, has not come into any wide practical application, from the fear entertained by medical and chemical authorities that possibly from some oversight a little barium might remain dissolved in the wine. But if M. Richet's conclusions are of a general value we should find here little cause for alarm. In fact, on the author's scale, 5 grains of barium, in solution, would have only about the same injurious action as 10 grains of magnesium!

In one very important point M. Richet agrees with Dr. Blake, with common experience, and, we venture to add, with common sense. He finds that the idea of poison is not qualitative, but quantitative. In contradiction to the rash dogma that whatever is poisonous in a large dose is,

pro tanto, poisonous also in the smallest, he shows that, as far as his experiments extend, all soluble salts—including those naturally present in the blood and taken in our food—are poisonous if administered in a sufficient dose. This truth does not come as a novelty to scientific physiologists, but to no small number of semi-scientific writers and orators it will be both new and unpleasant.

We have a further remark to add: the method of M. Richet—the immersion of fishes in water containing known proportions of deleterious matter—has been independently, and we believe from an earlier date, made use of by an English experimentalist, although from a different point of view, under which it is the only process possible.

IV. THE ETHICS OF INVENTION.

By An Old Technologist.

BEFORE attempting an examination of the questions involved in this subject, I must, as a preliminary, settle the distinction between the loosely-used terms "discovery" and "invention," which again presupposes a recognition of the difference between Science and Industrial Art. This difference, in spite of the full light thrown upon it by Whewell, John Stuart Mill, Sir J. Herschell, Comte, and others, the British public refuses to grasp. We still hear steam-navigation and railroads, revolvers and torpedoes, the Jacquard loom and artificial alizarin, and even the performances of "Farini's *Zazel*" and the "eagle-swoop" of Maraz spoken of as triumphs of Science. Yet the distinction is most simple and natural. The object and purpose of Science is simply to know everything that actually or potentially exists: it examines their properties, their mutual relations, and, not content with asking *what* exists, it proposes and seeks to solve the questions *how* it exists and what has called it into existence! But all this is done without any reference to human convenience or wishes, without any attempt to apply the knowledge gained to any purpose, good or evil. The results of Science are expressed not in receipts or working directions, but in phenomena recorded and in theories demonstrated.

With practical art, or what is sometimes called "applied science," the case is totally different. The object sought is not the increase of knowledge, but the promotion of material convenience, comfort, luxury, and power. Truths ascertained are valued not for themselves, but in so far as they admit of or promise some application. To this end the facts and the laws ascertained by Science are carefully scrutinised. Still it is an error to suppose that Art is a necessary consequence of Science. On the contrary, it is probably the elder of the two. Industrial processes were carried out at first empirically. Continued attempts led to certain results, though the principles upon which such results depended remained unknown for ages. As an instance I may mention the process of Turkey-red dyeing, which has been successfully carried out for a couple of centuries, but is even yet scarce fully understood. Art and Science are not necessarily connected with each other, either in place or time. A country might be rich in such men as Newton, Faraday, or Darwin: it might make rapid progress in pure Science, and yet its outward material existence might be poor and stationary, and its high thinking might be accompanied by very plain living. Again, modern advances in Art are not necessarily connected with any modern development of Science. Take, for instance, the revolver, which a smart writer in a daily paper not long ago took occasion to characterise as "one of the questionable gifts of modern Science." So far is this from being the case that a weapon essentially the same was in being about three centuries ago!

Science and Art, then, pursue respectively distinct objects, and with distinct motives. Every step in advance taken by the former is a discovery; every movement effected by the latter is an invention. Now a discovery, as such, has no moral bearings. The desire to know is neither virtuous nor vicious; not even between the two, but standing altogether on a different plane. Nor does the possession of abstract knowledge in the least modify human character. Suppose—and it is a fairly strong supposition—that I should next year decompose our present elementary bodies, and present to the world something worth calling a new chemistry. Yet neither I, the originator of the new discovery, nor any of the multitudes of people who would hear of it, fix it in their memories, examine the evidence on which it rests, and incorporate it in manuals and hand-books, would be morally one hair's breadth the better or the worse. But an invention has, or may have, a moral phase. It does something that was not done—could not be done—before, and that something

may be good or evil. The inventor, or would-be inventor, is therefore in duty bound not to overlook this question:—Will my invention, in its general and preponderating tendencies, be good or evil? Let us take a few instances. There is the Gillwell drying-machine, for securing the hay- and corn-crops in unfavourable seasons, as described in the October number of this Journal; there is the invention of Thomas and Gilchrist in the iron manufacture; the Le Blanc process for alkali-making; the various inventions for obtaining valuable products from coal-tar; there is the Nesbit process for rendering it impossible to tamper with cheques after filling up; and numbers of others which, whilst they increase the wealth and the resources of mankind, and give remunerative employment to multitudes of persons, do not put any new weapons into the hands of persons at war with society, whether such be criminal individuals, criminal associations, or criminal races. In all such matters there is no reason for the conscientious inventor to desist from experimenting in any particular direction, or to suppress results at which he has arrived. On the contrary, such inventors, whatever emoluments they may secure individually, confer far greater benefits upon their fellow-men, and may justly be regarded as having deserved well of the world. But there are other inventions, well known and important, where the benefits accruing are not without a serious counterpoise. As an instance of this kind let us take the lucifer match. Has not every rhetorical extoller of the present and contemner of the past pointed his moral and adorned his tale with these fizzing and somewhat evil-smelling emblems of progress? Nor am I about to call in question their fairly recognised advantages. There are conceivable cases where the life of a useful member of the community may hinge on the power of instantly procuring a light. But *per contra*. No one can deny that the "lucifer" has greatly smoothed the paths of crime. The rick-burner or other incendiary need not, as in the olden time, equip himself with flint and steel and tinder-box, and stand hammering away in a cold winter's night till he can secure the wherewithal for setting light to stack or barn. A simple box of matches saves him all this time, trouble, and risk of detection. The burglar need scarcely encumber himself with the proverbial dark lantern. Nor is this all: the match-manufacture, as hitherto carried on, undermines the health of no small number of people, and certainly withdraws a serious quantity of phosphorus from its legitimate duties as phosphoric acid in our fields and gardens. So that, in fine, though the good

produced by lucifer matches would probably outweigh the evil, yet there is a balance to be struck; and in any similar case it is, in my opinion at least, the duty of an inventor to weigh well both sides, and consider the consequences.

But I will pass to an instance where, I think, not the shadow of a doubt can prevail. A few weeks ago a paragraph was going the round of the papers concerning a marvellous invention said to have been made by an Austrian chemist. It was said—and whether truthfully or falsely it will for my present purpose matter little—that this man had invented a liquid compound which if flung upon a man deprived him instantly of all power and sensation, in which state he must remain till death unless restored to animation by a second liquid, also discovered by the same genius. I will pass over certain elements of improbability which on careful examination will appear in the story. It was then further alleged that the inventor had offered his secret to the Imperial Government, but instead of the hoped-for reward he had been commanded to destroy all memorials and records of his process, and to refrain from revealing it under appropriate penalties. Now let us suppose such an invention really made and revealed: the necessary result would be a perfect carnival of outrage. No man's life, no woman's honour, no property could be safe. The advantage of such a weapon as this fluid would be all on the side of the wrongdoer. Meeting any stranger in a lonely place, the only safety would be to shoot him down before he came near enough for the possible use of this diabolical compound. Suppose a respectable citizen walking home in a dark night: instead of the "stand and deliver" of the old footpad, or the hug of the modern garotter, against each of which a safe defence is conceivable, he would find himself without any warning asphyxiated, and would be plundered at leisure and left to die,—die, too, without any marks of violence to arouse suspicion. Or suppose any of my readers sleeping the sleep of the just in his own dwelling: a burglar, armed with this newest weapon, enters stealthily and bedews the face of the sleeper, whose "heart but once heaves and for ever turns chill." Against all these possible evils, which await nothing but the actual realisation of such a discovery to be rendered horrible realities, there would be but one set-off—the ease which such a man provided with this liquid could dispose of a tiger, a lion, or a grizzly bear. But this advantage would be far too dearly bought, at the price of putting such an irresistible weapon into the hands of the criminal class. Should any person accidentally come upon such a compound

it would be his most sacred duty to keep it an absolute secret, and the obvious policy of Society towards a man possessing such an invention would be to take one of the only possible guarantees for his silence.

But there are other inventions which, if not yet achieved, are eagerly aimed at, the effects of which would be scarcely, if at all, less dreadful. I may turn, in the first place, to aërial navigation. Not merely individuals, but associations, in different countries are busily engaged in planning, calculating, and experimenting, with a view to sooner or later travelling through the air. But no one takes the trouble to estimate the probable results, should success crown the efforts of these enthusiasts. Says one of the characters in Rasselas—"Were all mankind good and virtuous, I would with great alacrity teach them to fly." But in our days, as in those of Dr. Johnson, all mankind are very far from being good and virtuous. Nay, without asserting that the human race has grown more wicked, which may or may not be the case, it may be safely asserted that crime is now better organised, more intelligent, more on the alert to take advantage of modern improvements in the arts, mechanical, physical, or chemical. I will admit that the power of aërial navigation would be on some occasions and in some respects of exceeding value. I will take as an instance the exploration of unknown regions. Deserts, swamps, regions inaccessible from cold, would be no obstacle to the traveller, who, gliding along, could map out at his ease the country beneath him. Africa would soon have to surrender her remaining secrets, and the Polar regions would soon be as completely known as Switzerland or Norway. All this is an exceedingly tempting prospect, and I can scarcely wonder that those who do not take the time and the trouble to examine both sides of a question should become almost intoxicated at the very idea. And what is here the other side? Nothing more or less than that all the natural features and all the artificial appliances which from the prehistorical ages down to the present day have served to shelter individuals and nations from their enemies would be swept away at a stroke. Aggression would be indefinitely assisted, defence rendered almost impossible, and everyone who has anything to lose would be placed at the mercy of greed, of malice, or of a wanton disposition to injure!

This picture may seem at first sight overdrawn, but on close and calm examination it will be found correct to the smallest detail.

Let us suppose flying-machines invented, manufactured,

and offered for sale. A Peace of the days to come will find no more difficulty in purchasing such an appliance than he now has in buying ammunition or revolvers. Thus equipped he can sail through the air to the bank, the warehouse, or the mansion he means to plunder. What will it matter that all outer doors, all accessible windows are secured? Some upper window, some skylight, some weak point in the roof, or entrance leading to an inner court, heretofore inaccessible, will generally be found where the enterprising burglar may gain admission with little need for the tools of his trade. There will be a further advantage: at present the robber, going along the streets at night with his jemmy, his skeleton-keys, &c., is at once an object of suspicion, and is liable to be arrested on the mere fact of being found in possession of such implements. Again, suppose him in the present day, after a successful foray, going back to his den with a bundle of silver plate or other valuables on his shoulder. He is liable to be watched, followed, and seized. But his successor, thanks to "progress" and lovers of aërial navigation, will encounter no such difficulties. He will come and go unseen, his tools and his booty snugly reposing in his flying chariot as he sails along right over the head of the unsuspecting policeman. No one will have seen any suspicious-looking stranger in the neighbourhood. The deed being done, he may wing his way to San Francisco or Cape Town, to New York or Sidney, and there dispose of his plunder. It must surely be plain that the chances of detection after the deed would be reduced in as great a proportion as the facility of entering premises would be increased. Or take the case of murder: the intending assassin may arrive unknown at the place where his intended victim is staying, and may be gone before his presence has been even imagined, there being little more reason to suspect any one of the fourteen hundred million inhabitants of this planet more than any other. I would now ask—Are our appliances for the frustration of crime, and for its detection when actually committed, so superabundant, so more than equal to their task, that we can afford to give it such an immense additional advantage? That flying machines if once made could be confined to safe hands would be too much to hope. That the criminal class would feel any hesitation or reluctance to avail themselves of the new resources thus bountifully placed in their hands is unthinkable, especially if we consider how readily they have taken to the revolver, to chloroform, &c. Indeed our criminals seem far more alive to the value and possible utility of a novelty than do our manufacturers and merchants.

It may surely, then, be asserted that every man who has anything to lose has a deep interest in the non-success of the present attempts at aërial navigation.

But I must proceed further. In addition to the private criminal or the gang of criminals we have now what is facetiously called "private warfare" waged by secret societies against Governments. Easy-going simplicity is apt to fancy the leading spirits of such associations as loftier in their motives, and therefore entitled to more consideration, than the pirate or the brigand. A shrewder judgment will scarcely accept this view. Be this as it may, such societies would find the flying-machine solve all their difficulties. No fear would exist of their infernal machines being discovered by the officials of the customs or the police. Not to speak of what would ere this have happened at Berlin or St. Petersburg, where the sport of emperor killing would be much facilitated, it can scarcely be doubted that if the art of travelling through the air were known, London would before this have been in great part reduced to ruins and ashes. Surely a heavy price to pay for the facility of visiting the North and South Poles!

We now come to the third and last phase of the influence of aërial navigation, whenever effected,—that is, the modification which it would introduce into warfare. We in England have been hitherto in great measure protected from the worst horrors of war by the "silver streak of sea" interposed between us and the Continent. Large rivers, chains of mountains, and fortifications have hitherto frequently enabled nations to make head against a more numerous invader. All such defences, natural or artificial, would be at once sacrificed if the flying-machine becomes a reality. The nation which has the most numerous army, and which is constantly planning aggression, would be strengthened, whilst those nations who merely stand on the defensive would be weakened. It must also appear that when war is waged by sending flying-machines to hover over the cities of an enemy, and to let fall shells filled with nitroglycerine, the advantage will be all on the side of any country which has a scanty and scattered population, with little wealth stored up in any one locality. On the other hand, the richer, the more populous, and industrial a country, the worse it must fare. Suppose the French and the Tunisian Arabs each furnished with these appliances. Certain it is that the Arabs could inflict a hundredfold greater damage upon France than the French could upon their wilds and deserts. I have indeed met with a man

vehemently opposed to war as an unmixed evil, and yet, paradoxically enough, a perfect enthusiast for aërial locomotion. He freely admitted that railways, telegraphs, and other appliances which it was once hoped would fuse mankind into one peaceful brotherhood, had by no means had such an effect, and had even been utilised for the purposes of an invader. Still he hoped that with aërial navigation the case would be different, and that even if the horrors which I have attempted to point out should be realised, we of the present day ought to bear them contentedly, in the hope of some fancied "good time coming." Are my readers so devout in their worship of pseudo-progress that they will risk the utter overthrow of civilisation?

Somewhat similar in its tendencies and possible results is the proposal of submarine navigation, which is also a pet idea of my peace-loving friend.

I would ask now whether it is not a solemn duty on the part of inventors to turn their energies in directions where at all events there is no fear of promoting crime, treason, and aggressive war? All the inventive genius of our race might be exerted for a thousand years to come on subjects quite as remunerative for the individual, and much more advantageous to the public, without any such drawbacks. Are there not diseases and noxious animals to extirpate? Is there not the synthesis of plant-food and perhaps of human food from inorganic matter to be effected? Are there not summer rain-clouds to be dispersed, and cloudless night-skies in spring to be overcast? Are there not new sources of energy to be discovered, that we must needs employ our time and talents in playing into the hands of crime?

V. THE RECENT "VIVISECTION" CASE.

A few days ago the scientific and especially the medical world were painfully startled by the intelligence that application had been made at the Bow-street Police Court for a summons against Professor Ferrier, under the so-called "Vivisection Act." The prosecution—it might possibly be contempt of court if we were to use a

more appropriate term—was, at the instance of the "Victoria Street Society for the Protection of Animals from Vivisection"—a body whose *animus* and whose funds may be estimated from the fact that they engaged three counsel, a Q. C. and two juniors, to apply for the summons, a task which could have been quite as well performed by a solicitor, at the cost of one guinea.

As a matter of course the astonishment excited by the painful news was great. It seemed incredible that a man of the well known standing of Professor Ferrier, needless and insulting as he must deem the Act, would put himself within the grasp of the law by any neglect or violation of its provisions. Surely, it was said, if the learned Professor found it necessary in the course of his researches to prove any point by experiment upon a living animal, he would have applied for the usual certificate. An eminent medical contemporary, indeed, remarked that as the "Society" in question had not yet been able to catch any experimentalist committing himself, they had come to the conclusion that they must at any rate prosecute somebody by way of satisfying that part of the British public which has been persuaded to supply them with the needful funds. Our own version of the motives of the prosecution—as will be seen below—ascribes better strategy to the Anti-Vivisectionists.

The case came on for hearing on November the 17th, and in spite of the three counsel and of a great expenditure of sophistry, collapsed utterly. The summons with, what outside of legal circles would be deemed gross mendacity, charged Professor Ferrier with "having *on August 4th and on divers other days thereafter*, performed experiments calculated to give pain to two monkeys, and in violation of the restrictions imposed by the above Act." For all this it was admitted by the prosecuting counsel that the experiments upon the monkeys were performed six months ago, and consequently not at the time charged in the summons! The experiments, if we may so call them, which took place on or about the fourth of August, by Prof. Ferrier, could not be considered painful or held to come within the scope of the Act at all. He offered a biscuit to one monkey, which it took with the left hand in proof that it was paralysed in the right hand. The other monkey had been rendered deaf by the operations performed months ago, and in proof thereof Prof. Ferrier snapped off a percussion cap, of which the animal took no notice. These were the "shocking and frightful" experiments! The facts are that the experiments upon the monkeys had been performed some months previ-

ously by Prof. Yeo, in strict accordance with the provisions of the statute, and that he had obtained a licence to keep them alive, in order that the results of the experiment might be observed. On the day in question, in consequence of a discussion at a meeting of the Medical Congress between Professor Ferrier and Professor Goltz, a number of eminent scientific men, among whom we may mention Prof. Huxley, went to King's College, when the monkeys were produced by Dr. Yeo's servant. Professor Ferrier's sole offence, therefore, if it can be construed as such, consisted in his observing and pointing out the effects of an experiment performed months ago, and that under perfectly legal conditions. Yet Mr. Waddy presumed to call this "being present and taking a leading part in experiments (!) upon an animal upon which an injury had been inflicted." Rarely has the licence accorded to counsel of presenting facts in a manner calculated and intended to mislead been carried to a greater length. It is not, however, so much upon Mr. Waddy as upon those by whom he was retained and instructed, that the responsibility must rest. But of this anon. The magistrate, Sir James Ingham, very rightly remarked, that if noting the results of a past experiment constituted an offence, all those present in the laboratory of King's College were equally culpable with Professor Ferrier. How utterly untenable the prosecution felt their position appears from the fact that, though offered a case for the Superior Court, they did not accept the offer, a significant admission on the part of a body with whom money is evidently no object.

One question relates to the witnesses, Dr. Michael Foster and Charles Smart Ray. We do not for a moment entertain any suspicion of these gentlemen, the former of whom has been recently elected one of the Secretaries of the Royal Society, *vice* Prof. Huxley, resigned. But we think they owe it to the scientific world and to their own reputation to state publicly, that they did not volunteer their evidence, but appeared under *subpœna*.

We come now to the impersonal prosecutor—the "Society for the Protection of Animals from Vivisection," and its Secretary, a certain Mr. Adams. This gentleman and his Council or Board, or whatever name it may bear, are placed in a very unpleasant position. We do not mean legally, for despite the maxim current among lawyers, that for every wrong suffered there is a remedy against some one, we fear that Professor Ferrier would have very slender prospects of recovering damages from the prosecutors. But we refer to the position which they must occupy in the

judgment of all save the ignorant and the fanatical. Either they knew beforehand the facts which were brought out in evidence or they did not. In the former case they stand convicted of having wantonly and gratuitously subjected a man to the indignity and annoyance of a criminal prosecution, knowing that he had not committed the offences laid to his charge. In the latter event, if they did not know the facts they are then guilty of bringing a railing accusation at hap-hazard. If the contention raised by the counsel for the prosecution is ever upheld, it will become dangerous to look at an animal upon which any experiment has been performed at some antecedent date even under licence; it will be dangerous to note and discuss the results. We must further take good heed of the extension which the same counsel has attempted to give to the connotation of the word "experiment." It is perfectly plain that for the purposes of the Vivisection Act this word means exclusively some process or operation, mechanical or chemical, calculated to occasion pain to an animal. Yet Mr. Waddy speaks over and over again of "experiments" of a very different nature. Thus he says "it was in contravention of the fourth section of the Act to keep the animals alive for the purpose of having other *experiments* performed upon them." "The animal was brought over for further *experiments*." "An adjournment to the laboratory at King's College for further *experiments*." Now when we reflect that these "experiments" on which such stress is laid, and for which it was contended that a certificate ought to have been obtained, amounted to the offer of a biscuit and the snapping off a cap in test of deafness, the whole proceedings might be justly proclaimed a farce if they were less disastrous for the defendant.

We cannot help pointing out how different and how much more dangerous is the position of a man accused of inflicting pain upon animals in the pursuit of knowledge than that of one who has put birds or beasts to torture for gain, for "sport," or for the mere love of wounding and killing. Let us take the case of a cock-fight—a pursuit utterly useless and illegal under any condition. It is possible that the owners and backers of the cocks or any other person actually present at the fight might have to appear before the nearest petty sessions. The prosecution, however, would be conducted, not by a Queen's Counsel and two juniors, but by some local solicitor. Consequently the defendants would not be put to the expense of engaging on their behalf a corresponding array of legal talent. This feature, indeed,

is one of the most unworthy of the whole affair. It is, in fact, as if Mr. Adams had said to Professor Ferrier, "Whether you are innocent or guilty I will at all events put you to as much expense as I possibly can!"

There is here another feature; in the case of a cock-fight or a bull-baiting, none but those actually present and taking part in the performance are within the reach of the law. Suppose a wounded cock had survived the fight, no person examining him some six months after would be liable to a criminal prosecution. Now if we consider how much more severe are the penalties for unlicensed vivisection than for those cruel "sports" which have won for our country so unpleasant a reputation throughout the civilized world, it might surely be demanded that the provisions of the Act should not be laxly construed.

We turn now to the "Vivisection Act" itself. In the year 1876, when this Act was under discussion, some of our medical contemporaries expressed themselves satisfied with its contemplated provisions. It was considered that there was no reason to fear that the proposed measure would "in any material way" diminish existing facilities for research, whilst, on the other hand, a hope was expressed that it would "calm the needless apprehension, and put an end to the odious misrepresentations which have been recently rife concerning this subject, and which have been in ignorance adopted by persons of consideration, who will probably in future take more pains to be correctly informed." In the "Journal of Science" for July, 1876, in an article which the anti-vivisectionists have prudently declined to answer, we declared that these expectations would come to naught.

Our words have been more than verified. The agitation, far from subsiding, has increased in vehemence and in venom. An esteemed contemporary admits that it has been "making steady progress among the emotional and ignorant classes of the community," who are, in effect, our rulers, and pronounces, as do we, the passing of the Vivisection Act unfortunate and unwise.

The question now arises how long medical men and biologists in general can find it prudent to continue the policy which they have adopted since the commencement of the anti-vivisection uproar? How long are we to allow ourselves to be systematically traduced in pamphlets, petitions, speeches, and prayer meetings, without combining for the defence of Science? We have met and more than met all the charges brought against us, but our replies have appeared merely in medical and scientific journals which never reach

the general public, and which are on principle ignored by the "honourable men" who conduct the agitation. Hence the outside world is naturally led to believe that the accusations against us are true, and that we have no defence to offer. The position is really more serious than many of us seem to understand. The hysterical party who are organised to crush Physiological research in Britain, or if possible, in the world, are supported by "persons of consideration," whose wealth is only exceeded by their ignorance of the true bearing of the question. In the press they are powerful. At least one daily paper and one weekly journal, both of which have the ear of the present administration, are plainly against us. So are all the "Society" organs and the so-called "Christian" papers, thus exhibiting the curious spectacle of an alliance between Vanity Fair and Little Bethel—fashion and frivolity leagued with puritanism to suppress scientific research. In the same cause we see ecclesiastical dignitaries fraternising with dog-fanciers and sport-loving squires. In short, a great if not a preponderating portion of the most powerful interests in these realms are arrayed against us. Further, our opponents are organised whilst we stand isolated and ready to be hunted down in detail. For this purpose our enemies—it is no use disguising their real position—need only go on as they have begun.

Professor Ferrier, though the summons has been dismissed, has, in our opinion, suffered grievous wrong, and wrong for which he can obtain no redress. He has been put to very heavy expense, and what is perhaps to a scientific man still worse, to loss of time, anxiety, and distraction of mind. He will be held up to execration on sentimentalist platforms, and doubtless prayed at by the Holy Willies of Edinburgh. It may, perhaps, strike certain Honorary Secretaries that if scientific men can be thus annoyed, even though the cases break down, the ends sought may be gained. It will be perceived that this Act which counsel —we suppose ironically—described as "made for the benefit of the medical profession," and "framed so that there should be no restriction of experiments in the interests of Science," is fearfully elastic. That it has failed in doing what was amiably, or we might say fondly, hoped by its promoters, that is, in putting an end to the sensational agitation, is patent. As the matter stands it seems that merely looking at or making any observations upon an animal which has undergone an operation subjects the observer or spectator, if not to a penalty, still to serious

annoyance. We must, therefore, beg to propose that action should be taken without delay. The first desirable step is to call a meeting of persons interested, in order to adopt an address of sympathy with Prof. Ferrier, and to originate a subscription for the purpose of re-imbursing him for the outlay to which he has been put. Secondly and principally, a Biological Defence League should be immediately organised for the following purposes:—

1. For obtaining the opinion of eminent counsel upon the various provisions of the Vivisection Act, so that experimentalists may know when they are really safe and when they are in danger—which at present seems a very open question.
2. For instructing the public and the lay press on the real merits of the question by the circulation of tracts or by advertisements, &c., refuting the sophisms and exposing the inflammatory appeals of our opponents.
3. For petitioning the Legislature against any further restrictions upon physiological experimentation, and for rendering the granting of certificates a matter less dependent upon the caprice of the Home Secretary for the time being than it now appears to be.
4. For defending any person prosecuted under the Vivisection Act.
5. For counteracting and frustrating the system of espionage carried on by the Anti-Vivisection Societies.
6. For obtaining from all the Medical Faculties and Learned Societies of the United Kingdom, a formal Protest against the hysterical and inconsistent agitation now carried on.

There are many other duties which the proposed Defence League might undertake with advantage, and with a fair prospect of success, but to which it would be premature to call public attention. Surely in a matter of such importance all differences and distinctions should be waived, and biologists of all grades and views, medical or non-medical, evolutionists or advocates of distinct creation, animists or materialists, friends of official Science or of independent Science, ought to act together in unity. It must not be supposed that even the total abolition of experiments upon living animals will satisfy our opponents. They will next aim at the prohibition of killing animals for scientific purposes. The possession of serpents and other animals which require to be fed with living prey has been already objected to.

We need scarcely say that, whatever the "Journal of Science" can do towards carrying out the plan above suggested will not be wanting.

VI. THE PTOMAINES AND THE SNAKE POISONS.

IT will doubtless be known to many of our readers that, as far back as 1872, the late Prof. Selmi, of Bologna, succeeded in obtaining from the putrescent bodies of man and other animals a very interesting class of substances, to which he gave the somewhat awkward name of *Ptomaines*. These ptomaines were found to be not ferments, organised beings capable of multiplication, but well-defined crystalline compounds, bearing a very close analogy to the organic bases or alkaloids existing in the vegetable world. Some of them were fixed and some volatile, but each and all, if introduced into the animal system, were capable of occasioning the most alarming symptoms, such as dilatation of the pupils, followed afterwards by contraction, irregular action of the heart, stupor, tetanus, and finally death with the heart in systole. These affections, it was remarked, were very similar to those prodnced by muscarine—the active principle of the more poisonous Fungi.

The first sensation occasioned among chemists and medical practitioners on the announcement of these results was one of alarm, lest in chemico-legal cases a grave mistake had been, or might possibly be some day, committed. Certain of the symptoms produced by these ptomaines, such as the tetanic convulsions and the expansion of the pupils, are what might be expected to be produced by some of the poisonous vegetable alkaloids. The chemical reactions of the two classes of bodies were also found to be respectively similar. There was consequently the danger revealed that if a man was supposed to have been poisoned, his exhumed remains might on analytical examination yield compounds capable of being mistaken for the vegetable poisons, and that thus some person suspected of being concerned in the

death of the person in question might be wrongfully convicted.

Fortunately a method of distinguishing the two classes of poisons has been discovered. The ptomaines have a most intense reducing power. If a small trace of one of them is brought in contact with a trace of potassium ferricyanide—commonly called the *red* prussiate of potash—the latter is instantly converted into potassium ferrocyanide, the *yellow* prussiate. This change is at once known by the fact that the mixture after the reduction gives an immediate blue colour with a small quantity of a ferric salt (persalt of iron), the liquid being of course neutral or very slightly acid.

Certain of the vegetable alkaloids—such as hyoscyamine, emetine, igasurine, veratrine, colchicine, nicotine, and apomorphine—reduce, indeed, the ferricyanide, but less rapidly than the ptomaines. So far, then, we are freed from the terrible risk of being led by deceptive reactions to condemn an innocent person. At the same time it must be admitted that certain highly poisonous artificial bases—phenylic, pyrridic, hydro-pyrridic, allylic, &c.—behave with potassium ferricyanide, and with a subsequent addition of a persalt of iron, exactly like the ptomaines. Certain authorities, indeed, console themselves and the public with the reflection that these artificial bases have not yet found a place among the stock-in-trade of the criminal classes. We fear this is but a broken reed to lean upon, and it is therefore highly important that the properties of the ptomaines should be more thoroughly examined, so as to find some absolute method of detection.

It appears in the meantime that Dr. Gautier was engaged in the investigation of these carcase-poisons almost simultaneously with Selmi, and he has since greatly extended our knowledge of the subject. He finds that the ptomaines are formed not indifferently from any and every portion of the dead body, but solely from its albumenoid portions. What is much more important, he has proved that these poisons are not essentially and solely generated after death and during putrefaction, but occur in the normal excretions of man and of other animals, and in small quantities in most of our tissues. They are, in his opinion, necessary products of the process of dis-assimilation which is constantly going on in the tissues,—a residue, so to speak, of their life. The signification of this idea, theoretical and practical, can scarcely be over-rated.

In urine—not in any diseased condition, but in its normal state—two highly poisonous bodies have been not merely

detected, but isolated. One of these, first obtained and described by Dr. G. Pouchet in 1880, is highly oxidisable, possessing great reducing powers, forming well-crystallised double salts with the chlorides of gold and platinum, and if administered to animals killing them with tetanic symptoms. Along with this compound is found another body, nitrogenous, but not crystalline, not basic in its characters, and, like the former, highly poisonous. These discoveries throw a strong light upon the danger of a retention of the excretions in the body. The poisonous principles resulting from the waste of tissue require to be eliminated as rapidly as possible. If such removal is delayed or obstructed their accumulation must produce the most serious results, as we see in cases where, *e.g.*, the secretion of urine is suppressed. It has sometimes been noticed that the flesh of animals which have died immediately after unusual exertion, or after prolonged torture, is unwholesome, or even poisonous. This has been observed in the case of over-driven cattle, and of a baited bull. The most signal instance—the exact particulars of which we are unable to remember—is that of a roebuck which had been caught in a snare, and which had evidently died after very prolonged efforts to escape. All the persons who partook of its flesh became seriously ill, and, as far as we recollect, two of them died in consequence. In view of the researches of Dr. Gautier and Prof. Selmi these facts become perfectly intelligible. Prolonged and unusual exertion involves an abnormally great waste of time, and consequently an excessive production of ptomaines. If, then, the animal is killed and eaten before the elimination of the poisonous matter can have been effected, the persons who consume it are, as a matter of course, injuriously affected to a greater or less degree. The possible existence of these poisons in animal tissues may very probably be employed by the "dietetic reformers" as an argument in favour of vegetarianism. It must be remembered, however, that if we are to eat no substances which may contain traces of poisons, we shall probably have to extend our reforms so far as to dispense with food altogether.

But though the existence of the ptomaines furnishes no valid reason for the disuse, in food, of the muscular flesh of animals killed rapidly, when in a normal condition, the case is very different as regards the blood and the glands. Here, according to Dr. Gautier, we may find them in relatively larger quantities.

Dr. Gautier has sought for poisons of the ptomaine class in that part of the vegetable kingdom which makes the

nearest approach, chemically and physiologically, to animals, —*i.e.*, in cryptogamous plants. He finds that *muscarine*—the poisonous principle of certain Fungi—closely resembles the ptomaines both in its chemical behaviour and in its physiological action. It rapidly reduces the ferricyanide, and it kills with the heart in systole.

We next encounter another phase of the subject. The poison of serpents has been for thousands of years a mystery, both as regards its nature and its possible treatment. At last, thanks to the researches of Dr. Gautier, we are beginning to see light. He has obtained a small quantity of the venom of the lance-headed snake of Martinique (*Trigonocephalus*) and of the cobra (*Naja tripudians*). In each of these he has found an alkaloid, yielding crystalline hydrochlorates, chloroplatinates, and chloraurates, giving precipitates with the usual reagents for the vegetable alkaloids, rapidly reducing potassium ferricyanide, and, in short, possessing all the properties of the ptomaines. In the venom of the cobra there was also another compound of non-basic character, but exceedingly poisonous. We naturally ask whether this second substance is identical with that isolated by Dr. Winter Blyth, and named by him cobric acid? Dr. Blyth and Dr. Gautier certainly agree that the poison of the cobra is not a "figured ferment" or a virus, but a true chemical compound. As evidence for this view it appears that the venom may be boiled in aqueous solution, filtered, evaporated in the water-bath after acidulation, and filtered again without losing its activity, Further, the venom—which had been dry—was moistened with water, and heated for several hours to 120° to 125° C. (248° to 257° F.), yet its deadly power was scarcely affected. Moreover, its effects were diminished by a reduction of the dose and by dilution, just as would be the case with arsenic or prussic acid, and as is not the case with the ferments.

On the other hand, Dr. de Lacerda, who has been carrying out a long series of experiments with the Brazilian serpent *Bothrops jacaranda*, comes to very different conclusions. He states the poison lost its efficacy if heated to 90° C. (194° F.) or cooled down to the freezing-point. He also observed, under the microscope, minute briskly moving bodies, which were paralysed by an admixture of alcohol, chloroform, or boracic acid. Here is a perplexing discrepancy. We can well understand the presence in serpents belonging to different groups, of different poisons or of different ferments. But it is little likely that there should occur in one a true poison, and in the other a morbific ferment. Among the

substances which Dr. Gautier has tried without success are tannin, ferric chloride, nitrate of silver, a variety of essential oils, aldehyds, alcohols, phenols, and ethers, Ammonia delayed death a little, without otherwise affecting the result. A small quantity of dilute caustic potassa or soda was found successful.

This result is the more interesting as it is completely in harmony with that obtained by Dr. de Lacerda, in his experiments on the venom of *Bothrops jacaranda*. He injected into a vein of the animal from 2 to 3 c.c. of a solution of potassium permanganate, containing 1 per cent of the solid salt. Every chemist will see that the permanganate would be rapidly decomposed, and that caustic potassa would be formed. This injection was made upon animals which had received a dose of the poison, and which were already manifesting its characteristic symptoms, such as a great dilatation of the pupils, irregular action of the heart, &c. Yet in a few minutes after the injection of the remedy these symptoms disappeared, and there remained merely a general prostration, which did not last longer than a quarter of an hour. In blank experiments made upon other dogs, the poison being injected without being followed by the antidote, death invariably followed. Both Dr. de Lacerda and Dr. Gautier operated in such a manner as to exclude all sources of uncertainty. If we cause an animal to be bitten by a serpent question may arise as to whether the poison-gland might not for the time being have been exhausted, and, if not, whether the poison has been really introduced into the tissues of the subject. But both these experimentalists collected the venom of the snakes, and injected it into the subjects in exactly known quantities, so that no doubt can exist.

A very curious result is that the poison becomes much more active if previously digested with gastric juice at temperatures at from 20° to 39° C. (68° to 102° F.). On the other hand, the bile seems in all probability to have the power of destroying, or at least of delaying, the efficacy of snake-poisons. Popular tradition, in many countries, asserts that the gall of every poisonous animal is a remedy against its bite. We may now remove this belief from the region of superstitions to that of established facts, with this only reservation, that the gall of one animal is not more especially efficacious than that of another.

Dr. Gautier, after having thus established that the ptomaines, or substances closely analogous, are normal products of all animals, and after tracing them in the venom of

serpents, was not unnaturally led to seek for similar products in human saliva. It has long been known that a severe bite inflicted by a man or woman, especially in a state of rage or other excitement, has proved fatal. The salivary-glands in man are the representatives of the poison-glands of serpents. Prof. Oken used to declare that the saliva had the "signification of poison," and that one of its functions was to destroy the molecular life of the substances eaten. In human saliva a poison not inferior in activity to that of the more formidable alkaloids seems actually to exist.

Dr. Gautier obtained 20 grms. (nearly $\frac{3}{4}$ ounce) of normal saliva, and evaporated it down in the water-bath. There remained a residue weighing $\frac{1}{4}$ of a gramme, or nearly 4 grains. After being exposed to the temperature of 212° F. for three hours, this residue was re-dissolved in tepid water. If the liquid thus obtained was injected under the skin of birds the result was generally fatal, the symptoms being dilatation of the pupils and complete stupor. He has not yet isolated the poisonous principle of the saliva, but he finds that the watery extract prepared as just described immediately reduces the red prussiate of potash. Further, on treating the extract of saliva with a little dilute hydrochloric acid, and then with Meyer's reagent, a precipitate is obtained which, when washed and decomposed with sulphuretted hydrogen, gives a solution from which a hydrochlorate is deposited in fine microscopic needles. The solution of these minute crystals forms, with the chlorides of gold and platinum, crystalline but very unstable salts.

Hence we see that the venom of serpents is no exceptional anomalous product, but merely an intensified modification of ordinary saliva. We may now understand how it comes that animals, in very different parts of the zoological scale, up to the ruder portion of our own species inclusive, spit at an enemy. It might be important to subject the saliva of such animals—*e.g.*, the llama—to a special examination. Snakes are known occasionally to project their venom, and it is quite possible that such a spirt falling upon the eye, the lips, or the nostrils, might occasion serious results. We sometimes hear reports of illness, or even death, produced by the bite of some animal not officially recognised as venomous, and certainly not provided with hollow fangs. Such accounts are generally rejected as fabulous. But, making all due allowance for exaggeration and distortion, it is still probable that the saliva may, under certain conditions, be more highly charged with poisonous matter than is usual. Here careful observation is needed.

A further question may here arise: the saliva in all animals is constantly being secreted to a greater or less extent, and as constantly being swallowed. Why does it not occasion injury to the organism? It is no answer to this question to refer to the very similar case of the cobra, which in like manner swallows the overflow of its own poison-gland. The cause may probably be that the poison is destroyed in the intestinal canal by coming in contact with the bile.

From poisonous saliva as a general case we are naturally led to its especial form in rabies. On this subject Dr. Gautier has not experimented. The symptoms of hydrophobia, however, differ very strikingly from those produced by the ptomaines, by mushroom-poisons, snake-poisons, &c. One very characteristic difference is the time of incubation which the matter of rabies requires, after its introduction into the system, before the morbid symptoms are manifested, whilst the evil effects of the poisons above mentioned are rapid, if not instantaneous.

The poisons of insects, &c., ejected by a sting at the tail, do not for the most part seem likely to belong to the ptomaines. In the bees and the ants the venomous secretion is strongly acid, and has been proved to contain the formic acid. Whether its effects are in part due to any other principle is still an open question. In wasps, however, the venom, as was first demonstrated by Prof. Church, and as we have fully verified, is decidedly alkaline, and may possibly contain some compound of the ptomaine class.

As far back as the year 1872 Dr. Corre remarked that cases of poisoning from the consumption of poisonous fishes resemble entirely, physiologically speaking, those produced by the bites of serpents. Hence there is a strong probability that the tissues of such fishes contain substances chemically identical with the active principle of the venom of serpents and with the cadaveric poisons. To what end these poisons serve in most cases, or on what account they have been developed, is still utterly unexplained. We can see a reason why a cobra or a rattlesnake should possess a poison sufficiently powerful to kill its prey, and that rapidly. But it does not appear what the serpent gains by a poison so much in excess of what is neeeed for this purpose. The snake is not protected against the larger animals by this weapon, since if a man, a large dog, a tiger, &c., has been bitten, he has still ample time to take vengeance on the assailant. It seems, therefore, that serpent-poisons can scarcely have reached their present intensity by the action of Natural Selection.

VII. SEXUAL DISTINCTIONS AND RESEMBLANCES.

By FRANK FERNSEED.

IN the "Journal of Science" for 1878 (p. 469) there appeared an article on the "Woman's Rights Question," considered from a biological point of view. In that article the writer showed that the assignment of different duties and employments to men and to women respectively, which certain political and social agitators think proper to call the "subjection" or "subjugation" of the latter, so far from being peculiar to the human species extends throughout the great mammalian class, and when calmly considered, is merely an instance of that differentiation which is a characteristic of all progressive development. More recent researches have extended and completed his argument, carrying it out even into numerical details. It may, therefore, not be uninteresting to present a notice of the points of difference between the sexes—morphological and physiological—as summarised by M. G. Delaunay in the "Revue Scientifique."

We must first mark that in most of the lower sections of the animal kingdom the female is the larger, the stronger, and the better armed. As instances may be taken, many molluscs, especially the Cephalopods, many Cirripedia, most of the Annelida, and probably the majority of insects. Most distinctly is this the case among moths and butterflies, and no less among the Hymenoptera. Indeed, among the highest forms of this latter order, the bees, wasps, and ants, the male is unarmed—comparatively feeble; and as far as we can judge from the structure of the brain, not endowed with the same intellectual power as are the females and the neuters, which are now well known to be merely modified females.

In some of the Coleoptera, especially the Lamellicornes. the male is the larger and stronger, but this is far from universal. Among spiders the superiority of the female is most signal, the males in some cases being so diminutive that they have been, till lately, placed by systematists in different species and even genera. Even among vertebrates the female is still the larger in many fishes and reptiles. It

is not till we came to the warm-blooded animals that these relations are reversed. Among birds, save in a few predatory groups, now no longer placed at the head of the class, the male is the larger. This is most strikingly the case in the so-called Rasores or Gallinaceous birds, of which the common domestic fowl is the type. The case is very similar among aquatic birds. Thus the male swan, or "cob" as he is called, is much larger and heavier than the female, or "pen."

In the highest vertebrate class—if M. Minot will allow me still so to call it—that is, the Mammalia, the superiority of the male in bulk, weight, and strength is without exception. Nor is this difference in size the only distinction. In the male the phenomena of nutrition is more intensified. The blood is on the average denser and redder, containing more red corpuscules and hæmoglobine, and on the other hand fewer white corpuscules and less water. Malassey has even proved that man has in a cubic millimetre of blood a million more red corpuscules than has woman. Size for size, male animals eat more than do the females of the same species. I have known not a few men who have been quite uneasy concerning the small appetites of their wives or daughters, fearing it a sign of illness. It is proved from the experience of public charities that it costs more to feed a boy than a girl. Yet though women eat less, or, perhaps, for that reason they are inclined to eat more frequently. Of this the "five o'clock tea," intercalated between lunch and dinner, is an unwholesome instance.

With the work of the respiratory organs there is a corresponding difference. If we take a man and a woman of equal stature the capacity of the lungs in the man will exceed that of the woman by half a litre, or about 7-8ths of a pint. In the human species, according to Quetelet, between the ages of fifteen and fifty the female takes one inspiration more per minute than does the male, who, however, absorbs more oxygen than the female, and at all ages excretes more carbonic acid. Hence a hundred men will deteriorate the air of, *e. g.*, a lecture-hall or work-room, more quickly than will an equal number of women, and will consequently require a larger supply of fresh air. The temperature of the body is also measurably higher in the male sex.

If we examine the circulatory system, we find the pulse less frequent in man than in woman, but the pressure of the blood is stronger. The sexual difference of the number of pulsations in several species has been determined as follows:

—In the lion 18 beats per minute; in domestic cattle 10; in sheep 12; and in the human species, a number which varies from 10 to 14. Thus in the functions of animal life the male is distinctly pre-eminent.

The structural differences between the two sexes are by no means confined to mere height, breadth of shoulders, or depth of chest. The skeleton of the male is not only positively but relatively heavier than that of the female, bearing a larger proportion to the total weight of the body. The bones of a man contain a larger percentage of mineral matter and a smaller proportion of organic matter than those of a woman. Not only so, but there is a difference in the components of the inorganic portion, the male skeleton containing relatively more carbonate of lime and less phosphate of lime than that of the female. Livon asserts that the left shoulder-blade is larger than the right in women,—a difference which in the inferior races of mankind extends to both sexes. According to Broca the length of the clavicle in comparison with the humerus is greater in women than in men, just as it is greater in the negro than in the Aryan race. It is also said, though I doubt if the evidence is conclusive, that women use the left arm, and altogether the left side, more than do men. They are certainly more given to centripetal as contrasted with centrifugal movements than are men. Thus a woman, if cutting any article, brings the knife towards herself, whilst a man under similar circumstances cuts outwards, or away from himself, and is thus much less liable to hurt his hands.

In the voice there is also, in many of the higher animals, a sexual difference analogous to that which exists in our own species, the voice of the male being the deeper and more powerful.

In amount of muscular energy there is also a wide difference. The strength of an average woman in civilised countries is only about two-thirds of that of the average man. In the larger apes the difference is much greater.

I turn next to the skull, with its appendages and its contents. It is found that in the Aryan race women are relatively more prognathous than men, approximating in this respect to the condition of the lower races. The skull is decidedly more bulky in the male sex throughout all mammalian species which have been examined. Wiesbach determined its cubic contents in German women, and in men of the same nation, to bear the respective proportions of 878 to 1000. Morselli finds the difference somewhat greater, or as 850 to 1000. Broca states the excess of size in men, as

compared with women, to be 150 c.c. for the French generally, but as much as 211 c.c. for the inhabitants of Paris. There is also a difference in the shape of the skull, which is relatively longer and less elevated in women than in men.

The brain of men is heavier than that of women, not merely absolutely, but in proportion to the weight of the entire body. Thus the average size of woman bears to man the proportion of 927 to 1000, whilst the weights of the brain are respectively as 909 : 1000. Even when of equal size the brain in the female is lighter than in the male. The mean weight of seventeen male brains exceeded that of seventeen female brains, of the same size, by nearly 6 ozs. We find also relative differences of structure.

Broca, Wagner, and Huschke agree that in women the frontal lobes are less fully developed than in men, the difference in favour of men being, according to the last-named observer, as much as 54 c.c. In return the occipital lobes are bulkier in the female sex. It is considered by some authorities that the right brain is more developed in women, and the left in men,—a difference which, if established, would explain why in passing men generally keep to the right hand and women to the left.

From these data are drawn the following conclusions :—

"The superiority of the female sex is witnessed only in the inferior races of mankind and in the young children of the higher, and marks an inferior stage of evolution.

"The same is the case with the equality of the sexes, which occurs merely in imperfectly developed varieties and species, in young persons, in the decline of years, and in the lower classes of society.

"On the other hand, the pre-eminence of the male as compared with the female marks a higher stage of evolution. It occurs in the highest species and races, in the prime of life, and in the superior strata of society.

"Both morally and physically Evolution appears to have traversed from a state of superiority of the female to a state of superiority of the male, equality of the sexes representing a transition or intermediate stage."

These results, I fear, will not be welcome to the successors of John Stuart Mill. But when did a "reformer" stoop to consider such trifles as biological facts?

ANALYSES OF BOOKS.

The Human Species. By A. DE QUATREFAGES. Second Edition. London: C. Kegan Paul and Co.

WE must admit having deferred to a somewhat late point of time our survey of this important work, of which the excellent English version before us is now in the second edition. The primary idea of the author is to prove that all the beings commonly known as men belong to one and the same species, which was originally localised in a relatively very limited space, from which it has spread out over the whole globe by means of successive migrations. But the work has other and subsidiary objects, which, without any departure from the courtesy of criticism, we may characterise as belonging to the official school of French biology rather than to that which has established itself in the rest of Europe and in America. M. de Quatrefages is a disciple of Cuvier; he upholds the real existence of species, and consequently their permanence; he attacks the doctrine of Evolution; he denies the existence of permanently fruitful hybrids; he maintains a distinction between man and the lower animals, not merely of degree, but of kind. Thus in many of the most important points of theoretical biology he belongs to the first quarter of the present century. He admits, however, that man has existed upon the earth longer than is laid down by current traditions.

In the introductory chapter M. Quatrefages declares himself a vitalist. He does not accept the view—now becoming increasingly current—that the peculiar phenomena of living beings can be accounted for by mere physical and chemical considerations. His idea of life is "no more the *archeus* of Van Helmont than the *vital principle* of Barthez," but a name given to the "*unknown cause* which produces filiation, birth, and death." Here we are with him in so far forth that, pending actual proof of the production of organic phenomena by what are called inorganic agencies, we must pronounce it perfectly legitimate to speak of life as a distinct force. But we must here recognise a distinct breach in the principle of continuity. Until it has been distinctly demonstrated that the phenomena of the animal and vegetal worlds can in their entirety be explained by physical or chemical laws, we must admit that Nature has here done something *per saltum*. The author's views on this subject may be summed up in the statement that he ascribes the phenomena of the vegetable world to the "action of three forces—gravitation,

etherodynamy, and life." To the animal world he attributes, in addition, a fourth force—"the animal mind." Here, therefore, we have again another breach of continuity. The animal and the vegetable kingdoms are, according to M. de Quatrefages, not two stems from one common root, diverging more and widely from each other as they rise higher and higher, but two radically distinct totalities. It must even follow, on this view, that the lowest animal is superior to the highest plant.

But we come now to a more important violation of the principle of continuity. The author asks "Whether man should take his place in the animal kingdom? a question which evidently leads to another: Is man distinguished from animals by important and characteristic phenomena, absolutely unknown in the latter?" He continues, "For more than forty years I have answered this question in the affirmative, and my convictions, tested by many controversies, are now stronger than ever." Our reply is, "For more than forty years we have answered this question in the negative,—even on the hypothesis of distinct creation,—and our convictions, tested by the examination of all arguments raised on either side, are now stronger than ever."

One of our chief grounds for rejecting the doctrine of a great gulf between man and his "poor relations" is that scarcely two of its numerous champions advance the same reasons for their faith, and that each industriously and learnedly refutes the arguments of his fellows. We simply pay them the compliment of accepting their critcisims whilst rejecting their dogmatic teaching.

What is the evidence which our author advances in support of his assumption? We cannot expect that the learned academician would fall into the gross error of seeking to establish his theory by anatomical and physiological considerations. He says "It is neither in the material disposition nor in the action of his physical organism that we must look for these phenomena. From this point of view man is neither more nor less than an animal." Yet he makes afterwards certain remarks which can scarcely be brought into harmony with the great truth thus announced. For instance (p. 107) he tells us that "in man the organs are so arranged that he is essentially a *walker*, while in apes they necessitate his being a *climber* just as strongly." But what weight can be assigned to such a characteristic? In climbing and walking powers the apes vary greatly, and the gorilla, though he can climb, is much less arboreal and more terrestrial than the mias. Man, where circumstances permit it, is even yet an instinctive climber.

M. de Quatrefages further declares "Man also is evidently an exceptional or aberrant type among mammals. He alone is constructed for a vertical position; he alone has true hands and feet." The difference between the human and the simian type of hands and feet is so palpably one of mere degree that no ex-

ceptional position can be assigned to man on that account. The mere existence of the very decided heel-bone in the gorilla is quite sufficient to overthrow the foundation of the Cuvierian "Quadrumana." As for the vertical position of man, it is evidently simply the extreme form of a graduated series. All the apes when sitting have their vertebral column in an almost perpendicular position, and a gorilla when walking through bushes is very nearly erect.

To return to our author's survey of the distinctions between man and the rest of the animals, he admits that no line of demarcation can be found in the regions of the passion and emotions. He concedes also that man has true instincts. Nor does he find the boundary in intelligence, rightly pointing out that it is "not the *intensity* of a phenomenon which gives value to it from our present point of view, but simply its *nature*." He quotes the admission of the Pere de Bonniot, that animals possess a principle superior to mere matter, and to which we may give the name of mind; and that of M. l'Abbé Lecomte, that animals reason and judge. He claims for them innate sense and consciousness, and if he does not also add "reason" it is because of a circumstance to which he refers below. He rejects also Prof. Max Müller's alleged "Rubicon" between man and beast, showing that here also we have gradation, but not contrast. He goes on:—"In man the existence has been proved of fundamental phenomena, of which nothing, either in living beings or inanimate bodies, has hitherto been able to give us any conception. 1st. Man has the *perception of moral good and evil* independently of all physical welfare or suffering. 2nd. Man *believes in superior beings* who can exercise an influence upon his destiny. 3rd. Man *believes in the prolongation of his existence after this life*."

We must entirely traverse these alleged demarcations. We maintain that—

1st. The points advanced—two of them beliefs—are insufficient to warrant the inference sought to be drawn. Suppose it could be shown that all dogs or all chimpanzees entertained some belief—whether well- or ill-founded it matters not—which was not shared by the rest of the animal world; it would surely not warrant us in regarding them as fundamentally distinct.

2nd. It is not demonstrated, but merely assumed, that the lower animals have no perception of moral good and evil, and have no belief in the existence of superior beings or of a future state. On the other hand, there are phenomena which lead us to believe that they have rudimentary perceptions of right and wrong, and even suspicions of the existence of an order of Nature, any departure from which inspires them with a vague dread. But even in the absence of any distinct evidence to such effect, to follow M. de Quatrefages would be to found a classification upon our own ignorance!

3rd. Many men believe that our perceptions of moral good and evil have been gradually developed on utilitarian principles. Nor can we shut our eyes to the fact that multitudes of intelligent men consider the belief in superior beings and in an after-life a mere delusion.

But whilst thus rejecting the author's distinctions between man and the lower animals as utterly worthless, we must further express our opinion that the subject is here wholly beside the question. The unity or diversity of mankind is unaffected by such considerations as whether man forms a distinct kingdom, a distinct class, or merely a separate family or perhaps genus. What may have been the motive of M. de Quatrefages in beginning his task in the manner he has selected we have no right to pronounce.

In several of the following chapters the author is concerned with the reality and permanence of species, and with anti-Darwinism, or rather with anti-Evolutionist polemics. He here brings forward little, if anything, which has not been repeatedly advanced elsewhere, and has been found wanting. His characteristic defects here are a disposition to overlook or undervalue the palæontological and embryological evidence, and to cling to the notion of "filiation." In other words the author, if we do not misunderstand him, contends that because in the few centuries over which our observations extend, and in the few cases which have been studied with sufficient closeness and accuracy, we have seen plants and animals vary merely within certain limits, therefore species are permanent. This is the "Egyptian" argument, first raised by the members of the French scientific expedition to Egypt under the first Napoleon. It has been well remarked that this argument proves too much!

We cannot help here recording our protest against the spirit of many passages of the present work. He forgets that prejudices may be more easily Cuvierian than Darwinian. He speaks of men carried away by their enthusiasm, throwing overboard the results of their illustrious predecessors. Is he not aware that Darwin has been and still is one of the most patient and persevering observers and experimentators the world has ever witnessed? Are not his disciples, of whatsoever nationality, zealously following his example? Does not the balance of facts observed point so uniformly *against* the fixity and reality of species that the day for useful discussion is well-nigh over? In short, as an eminent German naturalist has well said, we no longer enquire whether transformation has taken place, but how it has been effected.

Anent the distribution of the human species M. de Quatrefages speaks of the centres of creation, or rather of appearance, "a doctrine entirely French in origin, having been formulated by Desmoulins and developed by M. Edwards." He might have added, "and entirely out of harmony with facts as now ascertained."

The researches of modern zoo-geographers supply no evidence of a number of independent centres of origin of animal forms. The facts speak in favour of successive waves of species spreading out from the northern hemisphere, and distributing themselves over the rest of the world according as channels of communication were closed or opened. If this section of the work has been recently revised by the author the circumstance is profoundly to be regretted. The author, criticising Agassiz, says:—"Agassiz includes New Guinea in the Australian kingdom. He thus destroys the homogeneity of the mammalogical fauna." This is a strange assertion if we consider that a species of that characteristically Australian genus *Echidna* has recently been discovered in New Guinea.

A prominent feature of the work is the position taken by the author on hybridism. Modern research tends to show that hybrids frequently originate in a state of Nature, and that they are not necessarily devoid of the power of reproduction. The hybrids produced between the domestic cattle and the American bison, which belong not merely to different species, but to different genera, are a remarkable case in point. It seems to us that the author assumes that two species in one genus must be as remote from each other morphologically and physiologically as two species in some other genus. Deny this totally unproven proposition, and the negative instance of the mule—upon which so much weight is laid by the naturalists of the old school—loses all its general value,

We find, on p. 143, an attempt to justify, or at least to palliate, the conduct of Cuvier as regards the discovery of fossil human remains in the year 1823. Says our author "The reproach is unjust. Cuvier had too often seen pretended fossil men change either into mastodons or salamanders not to be on his guard." Caution in forming judgment on a novel or possibly unique fact is doubtless highly commendable; but when that caution takes the form of refusal or neglect to give the disputed specimens a full and fair examination, it deserves, and generally obtains, a much harsher name.

Having disposed to his own satisfaction, and doubtless to that of official scientists in France, of these accessory questions, M. de Quatrefages proceeds to his main subject—the Unity of the Human Race. It may perhaps be maintained that the work would have had a higher permanent value had it opened with the Fifth Book. He shows in succession the possibility of man, even in a savage state, gradually overspreading the whole earth and becoming gradually acclimatised. He then treats of primitive man, of the formation of human races under the influence of the conditions of existence and of heredity, the origin of mixed races and the effects of crossing. He describes the fossil races, such as those of Canstadt, Cromagnon, and Furfooz. He next turns to the physical characters of mankind, anatomical,

physiological, and pathological, seeking in all these departments essential unity. In bringing into prominence the fact that all human races are subject to almost every disease, he might fittingly have extended his observations to the apes, who undoubtedly share many of our maladies, and under certain circumstances would doubtless be susceptible to them all. It is known that in the most unhealthy part of the season monkeys migrate away from the Teraï. In the last Book he traces the elements of unity in the intellectual, moral, and religious features of the entire human race.

The main, essential portion of the work is very much less open to criticism than the preliminary sections, and even those who do not admit that the human race might have sprung from a single initial pair may read it with pleasure and profit.

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*Das Princip des Westganzen und der Polarismus.** Von D. E. Mielke. Berlin: Theobald Grieben.

We have here, in brief compass, a far-reaching work. The author criticises the received scientific system, and proposes a substitute in its place, which he works out in astronomy, in the doctrines of atoms and cells, in botany and zoology, and in human thought, consciousness, and will. His fundamental principle is polarity. He considers that the mechanism of the heavenly bodies as commonly taught, as well as the hypothesis on the origin of the planetary system, due to Kant and Pezholdt (*i. e.*, the nebular hypothesis), appear unsatisfactory and "one-legged." The dualistic motor provided for the planets, consisting of the centrifugal and centripetal force, excites alarm lest the heavenly bodies, in consequence of total loss of heat, should perish from excessive condensation and from the shortening of orbits. The author considers it as unsatisfactory that the planets should be masses agglomerated out of chaos without internal organisation and polar activity. He considers that the problem of philosophy is to find the true mean between the subjective idealism hitherto cultivated and a materialism degenerated into the unnatural.

Turning by predilection to the chapter in which the author applies his principle to the organic, and especially to the animal world, we find him remark that plants and animals have this in common, that at one end the root or the head is organised for the reception of nourisment. At the opposite extremity the reproductive organs. But how about the many cases where the intersusception of food and the production of seed or ova are not placed at opposite extremities? We need only refer to the

* The Principle of the Universe and Polarism.

common snails and slugs, where the generative organs are placed in the neck, and to the earth-worm, where they form the so-called saddle about the middle of the body.

The author calls attention to the Lichtenbergian figures of positive and negative electricity in comparison with the human nervous and cerebral system. He places, again, the brain with the organs of sensation as one pole of a system, whilst the generative organs form the opposite. As a second system he adduces the heart and circulatory system with the respiratory system and the vocal organ. The third main system of "two-ended and two-sided" activity is the muscular tissue. The author considers that plants bear the same relation to the earth's surface that hairs do to the animal. The vegetable world dates back to a remote past. Among physiologists it is possible to agree with those only who accept equivocal generation as a cosmogonic appendage.

Having thus given a short sketch of some of the author's leading doctrines, we must pronounce the work as indeed suggestive, but somewhat deficient in clearness.

On the Teaching of Science in Public Elementary Schools. By W. J. Harrison, F.G.S., Science Demonstrator for the Birmingham School Board. Birmingham: Herald Printing Office.

We find in this pamphlet very much which commands our most hearty approval. The attempt to give even primary education a more re-al—we divide the word to remind the reader of its etymological meaning—instead of its present exclusively verbal character is deserving of every encouragement. It is generally supposed that the main motive which induced the Government of this country to undertake seriously the task of national education was the spectacle of other countries gaining ground on us, and seriously challenging our commercial and manufacturing pre-eminence. But both Government and nation forgot that no amount of the three R's can, from the very nature of things, meet the exigencies of the case. The youth of this country should, we submit, be trained in the art of observation, so that they may learn a lesson from every object and every fact which they come across. This training has been till lately at least conspicuous by its absence in every grade of English education from the village school to the university, and the many illustrious discoverers and inventors to whom this country owes so much of its greatness have risen not by the aid of what they had learnt, but independently. Hence it is that we attach so great value to the attempts of which this pamphlet speaks. The writer is fully

justified in pronouncing the scheme of teaching—*e.g.*, Natural History—in the guise of reading-lessons a grave error. True, the leading facts of Natural History may thus be somewhat dimly implanted in the minds of children, but they will never on this system be taught to observe and to draw correct inferences from the phenomena around them. To learn any branch of the natural sciences rightly we must go to things, and make use of books merely for recapitulating and systematising our knowledge.

The plan adopted in Birmingham of having a Science Demonstrator who attends the schools to give lessons in rotation, and has the materials requisite for illustrative experiments taken round with him, is an admirable plan for combining economy with efficiency. We are happy to learn that the children show a lively interest in the lessons.

Osteology of Lanius Ludovicianus var. *Excubitorides*. By Dr. R. W. Shufeldt.

We have here an osteological monograph of the variety *Excubitorides* of the shrike, *Lanius Ludovicianus*. The characters of this bird, as a type of its group, are beautifully displayed in the accompanying plate of its skeleton. Here, as the author remarks, we see the general framework of an ordinary insessorial bird, which might belong, *e.g.*, to a thrush, yet surmounted by a raptorial skull, approaching that of a hawk. We seem to see here a transition not yet completed. Dr. Shufeldt does not indicate any other raptorial feature save the "decided curvature of the bird claw," but we may possibly lay too much weight upon this characteristic from our knowledge of the habits of the bird. It is singular how few birds are either purely vegetarian or purely carnivorous. It has been remarked that some of the most decided fruit-eaters turn predatory in severe seasons, and even attack their own kind. The idea of a strictly defined "natural food" for any animal species is fading away like that of species themselves.

CORRESPONDENCE.

*** The Editor does not hold himself responsible for statements of facts or opinions expressed in Correspondence, or in Articles bearing the signature of their respective authors.

A NEW SEISMOGRAPH.

To the Editor of the Journal of Science.

SIR,—I notice an article in "Nature" for November 3, 1881, headed "Lunar Disturbance of Gravity." Of course it is possible, as I know to my cost, for a new theory to originate with the printer's boy; but the gist of this paper is evidently the description of a delicate seismograph, which reflects great credit on its constructors. Indeed it is notable there is no effort put forth to establish the theory of a disturbance in *a*—or *the* if it be land-tide as eliminated from extraneous circumstance during the course of the paper, the matter being left among the desirables stated at its commencement. One fact, however, is duly arrived at, and circumstantially stated towards the close; namely, that the effect of frost and a wet season combined is strongly marked, for on January 23rd, 1881, a foundation-stone was 4·12 m.m. higher than it was on September 7th, 1880; and also that the prolonged drought of the present summer (this must refer to England, for it has been a proverbially wet year in Scotland) has had a great effect, for between May 8th and June 29th the stone sank through 5·79 m.m. This matter then, I suppose, is quite undeniably Solar Physics proper, and it should tend to place, without the bourns of mere theoretical assumption, the notion that superficial earth tremours may be so produced. Indeed the writer of the paper would lead one on yet farther, for he adds:—The changes produced in the height of the stone are, of course, entirely due to superficial causes; but the amounts of the oscillations are certainly surprising, and although the basements of astronomical instruments may be very deep, they cannot entirely escape from similar oscillations." This I suppose to refer to the leverage exerted by the expansions and contractions in the crust of variable temperature, which, as Page tells us in his "Text-Book of Geology, should be taken to be 60 or 90 feet in vertical depth. Indeed, as I understand, it has been already mooted to

place this new seismograph at the bottom of a mine; but can any assure us that even then no tremour of the earth shall there penetrate, and that the astronomer shall see nought, hear nought, feel nought, but the gentle influence of Astarte. This will surely prove a knotty point for the mathematician and mechanic.—I am, &c.,

A. H. SWINTON.

Binfield House, Guildford, November 15, 1881.

HYLOZOISM *versus* ANIMISM.

To the Editor of the Journal of Science.

Communicated by Dr. LEWINS.

SIR,—I venture to request the insertion of a few observations upon Mr. Barker's article in the October number of this Journal. The motto "God is light" appears to me a suicidal one, since light, on either theory of its nature, is a purely material force. Plato, Socrates, &c., are quoted in support of Animism; but it is well known that the existence of "spirit" as an immaterial entity was as yet undreamt of in ancient philosophy. I do not assert that Matter is "what men in general call Mind," but that mind is a function of matter, and surely the word "function," which signifies office, duty, or operation, is neither vague nor ambiguous. Hylozoism is independent of the Lucretians and of every other atomic theory, while it includes and completes the idealism of Berkeley, as implied in the sentence which Mr. Barker declares "unintelligible." The doctrine of "centres of force," which places mental and vital energy on the same level with heat, light, and electricity, is only another mode of stating the same thesis. As Berkeley affirmed, all phenomena are the product of mind—*i.e.*, of thought and sensation; but these powers must belong to some conscious entity. The grey matter of the cerebral hemispheres (of whose noumenal nature we are of course entirely ignorant) has been proved by modern research to exercise mental functions, and to be, therefore, the proplasm of the mind, or "subjective universe." Farther we cannot penetrate, but are fairly entitled to say—the Brain thinks, *ergo* it exists. The volitions of man are, in the last analysis, as purely due to physical causes as the fall of a stone. As regards my argument, the etymological signification of "vis insita" is totally unimportant. Practically it means immanent, innate, or inalienable. No flippancy was intended by the phrase "ghostly Archeus;" ghostly being the Anglo-Saxon equivalent of "spiritual" and "Archeus," the normal term in pre-scientific Physiology to denote the imaginary and occult active principle of the material

world, and the power "behind" organic function that presides over "the animal economy." The illustration drawn from the old phlogistic theory of combustion remains intact. There was no *analogic* ground for supposing a "levitating factor" which "could have united with a metal without being condensed into a solid." Neither is there any such ground for the belief in an "anima" or "vital principle." Again, when a man is excited by alcohol he manifests a temporary exaltation of vital, and often of mental, power; but nothing analogous to this happens in the case of a broken fiddle. All discussions based on theological dogmas are out of place in a scientific journal; I therefore simply remark that if we assume the existence of an *ubiquitous* Deity—an assumption Hyloism is not concerned to dispute—we must believe that he acts in all cases directly, and not by living or lifeless mediation,—

> "Warms in the sun, refreshes in the breeze,
> Glows in the stars, and blossoms in the trees,"—

and that the material Universe is but the Epiphany of God. Is this a "degrading" theory?—I am, &c.,

C. N.

PROFESSOR HUXLEY ON ANIMISTIC HYPOTHESES.

To the Editor of the Journal of Science.

SIR,—Professor Huxley, in a Lecture delivered before the Medical Congress, asserts that "The essence of modern, as contrasted with ancient physiological science, appears to lie in its antagonism to animistic hypotheses and animistic phraseology."

Prof. Huxley is never ambiguous in his utterances, and it is very difficult to avoid applying an animistic hypothesis to the function he was fulfilling in this portion of his admirable address. I would, however, suggest that the opposition of Science is directed against *inadequate* rather than against *animistic* hypotheses.

For example: the hypothesis that the process in germination is similar in its nature to that in crystallisation is grossly inadequate and misleading, and, as such, is condemned by Science.

It is not Prof. Huxley's custom to complain of that which is inevitable; and it is to be feared that in our modes of thought and expression we cannot help being animistic, any more than

we can help being anthropomorphic. In Biology 'mimicry' cannot be discussed except in terms which belong to animism. The remedy is in the Professor's own hands: let him show that all animistic potency is mechanical, and the offence ceases.—I am, &c.,

HENRY H. HIGGINS.

OCCURRENCE OF EARWIGS.

To the Editor of The Journal of Science.

SIR,—S., at page 690, writes of the *abundance*, "almost *beyond precedent*," of earwigs. As an old grower of roses, carnations, and dahlias, for over twenty-five years, I know an earwig when I see him (which is death to the earwig), but this summer—strange to me, as doubtless it will be to S.—*I have not seen a single one.* I grew a hundred pots of carnations, and more than once remarked on the absolute freedom from the "nibbling thieves." My garden faces North, on a steep hill-side, and I added no manure to my soil.—I am, &c.,

T. C.

NOTES.

ACCORDING to "Light" Prof. Schlager, of Vienna, has been experimenting for three years on the influence of blue light in the treatment of the insane. He records a quieting and soothing influence in cases of an abnormally excitable temperament.

The number of persons killed on railways in England is proportionally twice as great as in France.

"Les Mondes" of October 6th contains an Essay on the Alimentary Prescriptions of the Mosaic Law and Modern Science which, in the main, agrees with the article on the Sanitary Legislation of the Pentateuch contained in our October number.

Dr. C. Doremus gives an analysis of the milk of the elephant, which appears to contain less water, and more fatty matter and sugar, than that of any other animal.

Dr. H. George ("Les Mondes") gives several instances of living animals which had penetrated into the human ear, and occasioned alarming symptoms.

A Life of the late Prof. de Morgan, by his widow, is about to be published by Messrs. Chapman.

One of the most curious phenomena of the age is the recrudescence of Asceticism, which seems to be forming an alliance with Spiritualism, as but too plainly manifest in the organs of the latter. Thus Dr. G. Wyld, in the "Psychological Review," recommends "occasional reasonable (*sic !*) fasts.

H. Hoffmann ("Botanische Zeitung), after experimenting for twenty-five years on the variation of plants, admits that in the course of his researches he has gradually lost the idea of species, for which he can find no characteristics. He inclines to the opinion that variation takes place in definite directions, and that its cause is in a predominating degree internal.

Mr. A. Agassiz well remarks that "the pupil studies Nature in the school-room, and when he goes out of doors he cannot find her."

A series of circumpolar stations for simultaneous magnetic and meteorological studies is about to be established by a number of States. Britain takes no part in the matter.

According to the experiments of Prof. Forbes and Dr. Young the speed of the blue ray of light exceeds that of the red by about 1 per cent.

M. l'Abbé Moigno writes ("Les Mondes," Oct. 6th), "Le jeune animal sauve sa vie, l'enfant au berceau meurt sans sa mère." He forgets that the young of the higher apes are as helpless as human children.

The value of potassium permanganate as a remedy for snake-bites is ably discussed in the "Medical Press and Circular," and pronounced not proven.

On October 26th we had the pleasure of visiting the Aylesbury Sewage Works, where the process of the Native Guano is being carried on with striking success. The exhibition of agricultural and horticultural produce grown with the sewage manure was pronounced by competent authorities most satisfactory.

At the November meeting of the Entomological Society were exhibited certain tubes constructed of weathered dogs' dung by a Lepidopterous larva, of a species as yet not determined. This is the first decided instance of coprophagous habits in the entire order.

According to M. A. Laveran ("Comptes Rendus") marsh-fevers are occasioned by the introduction into the blood of parasites, which the author describes at length. The curative action of quinine is due to its being a poison to these intruders.

Sir Wyville Thompson will shortly vacate the chair of Natural History at the University of Edinburgh. It is hoped that Dr. Alleyne Nicholson will be appointed as his successor.

Dr. J. Burdon-Sanderson pronounces the "final cause of an animal, whether human or other," to be muscular action.

Profs. Rutherford and Greenfield, in opening their classes at the University of Edinburgh, referred to the value of vivisection, and exposed the folly of the present sentimental agitation for its suppression.

MM. Arloing, Cornevin, and Thomas have investigated the well-known immunity of adult cattle from bacterian anthrax in the infected districts. They ascribe this immunity to a gradual and infinitesimal vaccination which they have undergone, since aged cows and oxen, from districts where anthrax is not common, if brought into an infected part of the country succumb as readily as calves.

This summer some common hive-bees built a comb against the flat wall of a house in Dorsetshire. Some pieces exhibited at the November meeting of the Entomological Society had evidently contained bee-grubs. This is an unexampled departure from the habits of the species.

Prof. Helmholtz makes the following significant remark:—"Our generation has suffered under the influence of spiritualistic

metaphysics: the coming one will have to be on its guard against materialistic philosophy."

"Les Mondes" notices an interesting case of melanhæmia. A French soldier who formed part of the army of occupation of Rome, and was stationed for some years in the Pontine Marshes, has become entirely of a deep brown colour, totally distinct from the shade produced in natives of Northern Europe by exposure to the sun in lower latitudes.

"Light," referring to the case of Mrs. Croad mentioned in our September issue, states that a Miss Fancher, of New York, though blind, could "read a book by running her fingers over the pages, and do the most elaborate fancy wool-work, involving the nicest discrimination of shades of colour, in pitch darkness."

According to MM. Dufour and Forel ("Soc. Vandoise des Sciences Naturelles") the glaciers of the Alps, Pyrenees, the Norwegian mountains and the Caucasus, as well as those of Greenland and Spitzbergen, have been receding. The study of this phenomenon may probably throw light upon the causes of the Glacial epoch.

We consider it our duty to reproduce the following notice from the "Journal of the Society of Arts":—"It having come to the knowledge of the Secretary that circulars, purporting to be issued by 'The Society of Science, Letters, and Art, of London,' or some similar title, and dated from Finsbury Park or Upper Tollington Park, have been sent to certain Members of the Society of Arts, inviting them to subscribe to the 'Society of Science, Letters, and Art," and that several subscriptions have been paid to the Secretary of the above Institution under the impression that it was connected with the Society of Arts, he is desired to give notice that nothing whatever is known of such a Society at this Office, and that it is not associated in any way with the Society of Arts."

M. Fredericq ("Bulletin Belg. Acad.") has examined the blood of the larva of *Oryctes nasicornis*. It is colourless, but gradually turns brown and coagulates on exposure to the air. It does not appear to contain any body like hæmoglobine or hæmocyanine which serves as an intermediary between the atmospheric oxygen and the tissues.

Mr. W. C. Holbrook, in a paper read before the American Association for the Advancement of Science, describes the skeletons of the "mound-builders" found in Rock River Valley. The cranium is small, low, and broad; traces of a frontal suture are found even in the skulls of adults. In about 50 per cent of the humin there is found a well-developed foramen, larger and triangular in the older bones, but in the more modern small, circular, and less frequent.

The following extract from microscopical observations made by Stephen Gray in 1696, and quoted by Mr. W. S. Kent in his "Manual of the Infusoria" now in progress, would seem to foreshadow the important modern discoveries of homogeneous immersion and immersion illumination:—"Having by me a small bottle of water, which I knew to have in it some of those minute insects which the deservedly famous observator Mr. Leeuwenhoek discovered by the help of excellent microscopes; having seen them with the common glass microscopes, and with the first aqueous* as above mentioned, I poured a few drops of this water on the table, and taking a small portion thereof on a pin I laid it on the end of a small piece of brass wire, of about one-tenth of an inch diameter. I continued to lay on two or three portions of water, till there was formed somewhat more than an hemisphere of water; then keeping the wire erect I applied it to my eye, and standing at a proper distance from the light I saw them and some other irregular particles, as I had predicted, but most enormously magnified; for whereas they are scarce discernible by the glass microscopes, or the first aqueous ones, within the globule they appeared not much different both in their form, nor less in magnitude than ordinary peas. They cannot well be seen by daylight, except the room be darkened, after the manner of the famous dioptric experiment, but most distinctly by candle-light: they may be very well seen by the full moon-light, and the pin sometimes takes up the water round enough to show its objects distinct." Here the side of the spherule of water next the eye acts as a plano-convex lens, magnifying the animalcule in the centre of the sphere, the opposite half doing duty as an immersion condenser, and here the matter rested and was forgotten for one hundred and eighty years.

The following rough and ready method of cleaning diatoms is given in the "American Journal of Microscopy" (ii., 1881, p. 93):—"A few crystals of bisulphate of potass are crushed, and a sufficient portion added to the material to be cleaned. The mixture is placed in a hollow scooped in a sound piece of charcoal, and heated before the blowpipe until it ceases to fuse readily: when the substance appears to be opaque, and of a whitish colour, it is to be removed, dropped into a small quantity of water, and boiled for a few seconds. The diatoms and sand are now liberated, and can be washed and separated by any of the usual methods." It is claimed for the process that it saves the troublesome boiling in acid, and that the apparatus and materials are easily procurable.

* A spherule of water held in a loop of wire, or a hole in a metal plate.

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